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THE JOURNAL
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EDITED BY
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PSYCHOLOGICAL RESEARCH IN AVIATION IN ITALY, FRANCE, ENGLAND, AND THE AMERICAN EXPEDITIONARY FORCES

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Among the many demands for the application of psychology to practical problems, those presented by aviation are of particular interest. In 1914, very little, if anything, was known regarding the qualifications of an aviator, aside from the fact that it was assumed that he must possess an unusual amount of daredevil spirit. As late as 1917, it was supposed, in this country at least, that the demands upon the aviator were totally different from those made by any other work. The qualifications in balance in the air, as contrasted with balance in walking, quickness in reaction, emotional stability, and good judgment, were supposed to be much superior to those for a gunner, a signal corps officer, or a lieutenant in the trenches.

While it is doubtless true that we have learned that flying does not require the unique qualities which mark the aviator as a superman, nevertheless great steps have been taken in the direction of perfecting methods for the selection of those young men who give promise of learning this particular type of performance most successfully. This work has been performed by medical men, physiologists, and psychologists. While in this country the work of these three groups has been kept quite distinct, the research abroad has not been so definitely divided.

ITALIAN RESEARCH

Italy was the first of the allies to take up extensively research in aptitude for flying. This work was carried on at psycho-physiological laboratories at Turin, Naples, and Rome, under the general direction of Dr. Gieuseppe Gradenigo. Here were developed a number of tests, and a mass of data gathered in routine examinations and in intensive research on selected groups of aviators at the front as well as candidates for aviation instruction.

In preliminary work, a number of tests were tried out upon pilots known to be successful aviators, upon another group rated mediocre, and upon a third group of unsuccessful aviators. The conclusions reached were that "a good airplane pilot is one who to a sufficient speed of perception and to a notable degree of extension and distribution of attention, adds constancy, precision, coördinating ability of the psycho-motor activity, and who possesses a sufficient inhibitory power of emotive reactions not to be disturbed in the above functions on account of emotional stimulus." In accordance with these conclusions, tests were sought that would measure psycho-motor activity, attention, and resistance to emotional stimulus. An account of this work is given in "Richerche biologiche sull' aviazione," Rome, (Tipografia Nazionale Bertero) 1919, pp. 235. These experiments covered studies in reaction times, attention time of perception, emotional stability, muscular sensation or perception of muscular effort, and equilibrium, as well as the more distinctly physiological and medical aspects.

Simple reaction times with visual and auditory stimuli were used in all the laboratories. Of 13,936 candidates examined in 1918, 247 were disqualified on the reaction time test. In the earlier report by Gradenigo the limit of fitness was placed at 0.200 second for the visual and at 0.170 second for the auditory stimuli, with a variability not to exceed an average of 0.030 second.

Saffiotti (p. 159) reports more in detail the method adopted at the Turin laboratory. The purpose of the tests is stated to be the determination of the "functioning of neuro-muscular processes during a period of attention, at which time an individual tends to show his perceptive-motor reactions." He claims that the objection usually made to simple reaction time tests, that is, that automatism in the reactions arises, is not founded. The fact that most of the series in all three laboratories consisted of only twenty readings, preceded by a few preliminary practice reactions, would lend weight to his view.

The reactions were recorded with a Hipp chronoscope, arranged for a break-make circuit with the use of the lower spring only, the current passed through the upper coils. The average errors were 0.001 to 0.002 second. The stimuli adopted were, for visual, a green diaphragm lighted by a 2 candle power lamp; for auditory, a lever falling from a fixed height striking a key. Having explained the task to the subject, the reactions are taken until "a normal, homogeneous group of twenty reactions" are secured. Usually the subject reacted immediately with normal reactions, though occasionally it was necessary to spend some time to accustom him to the method. The stimuli in the series seemed to have followed rather rapidly and no "ready" signal given.

The values obtained range between 0.170 and 0.200 second with the visual stimulus, and 0.130 and 0.150 second with auditory. Values above these are considered slow and the individual is rejected. Saffiotti claims there is no reason for having placed the maximum limit of tolerance with the auditory stimulus at 0.170 second as reported by Gradenigo, but he gives no definite reason for reducing the limit to 0.150 second. He suggests, however, that the elimination should be on a 10-percentile distribution, rather than on any fixed arbitrary standard. On this basis the average deviation of 0.030 second was considered the limit of variability.

Choice reactions were made with right or left hand in either of four directions and combined reactions with hand and foot. The technique of the latter is described (p. 210) by Azzi of the Naples laboratory. In a frame were arranged five 16 candle power lamps, two upper, two lower and one in the center. Below each lamp was a small lamp of the flash-light variety. Under this bank of lamps was a table with two keys for the hands, and on the floor under the table, two keys for the feet. The subject was instructed to react with the hand to the corresponding upper lamp, and with the foot to the corresponding lower lamp. He might be required to react to any two lamps simultaneously. The small lamps lighted when the subject reacted, thus indicating whether he reacted correctly. A D'Arsonval chronoscope was used. The following are given as the normal values:

Average reaction time	0.500 second
Average deviation	0.120 second
Coefficient of variability	20
Percentage of errors	8

The following were considered abnormal values and standards for rejection:

Average reaction time	0.680–0.700 second
Average deviation	0.220 second
Coefficient of variability	36
Percentage of errors	26

Gemelli states (p. 51) that the reactions with the left hand, whether in isolation or simultaneous with another member, are more prompt than the reactions with the right hand.

The reaction apparatus used by Saffiotti (p. 182) and by Romagna-Manoia (p. 212) consists of a vertical lever which is grasped by the subject, as is the stick control of the airplane, and which can be moved forward, backward or to either side 1 cm. from the center. The stimulus is given by a lighted arrow indicating the direction of the movement, and by a negative command (not described) to which the subject must not react. The results centralize between 0.350 and 0.500 second with little difference in the various directions. The variability oscillates between 0.040 and 0.070 second, and since from 0.070 to 0.080 second there is a decided decrease in frequency, 0.070 second is considered the greater limit of variability, or standard of rejection. There were no errors in reaction, except for an occasional failure of inhibition with the negative stimulus.

Romagna-Manoia, using the four movement stick for reactions, but using four lamps, arranged above, below, and on either side of a fifth lamp, as stimuli denoting the direction of the reaction (the central lamp not to be reacted to), obtained the following results from four hundred candidates (p. 217):

DIRECTION	MEAN (SIGMA)	EXTREME VALUES	MEAN VARIATION	COEFFICIENT OF VARIABILITY	PERCENTAGE OF ERROR
Forward	453	326–633	60.20	13.28	0.85
Backward	478	344–663	57.97	12.12	0.50
Left	491	356–669	58.50	11.91	0.65
Right	465	346–659	55.10	11.84	0.72

In the cases of nervous exhaustion from service at the front, the simple reaction time method was applied by Saffiotti "for purposes of study of the state of the nervous system and the capacity of attention" (p. 183). These series consisted of 150 to 300 reactions with the visual stimulus, and 100 to 200 with the auditory. Saffiotti claims to have been able by repeated tests "to follow the manifestations of the progressive renewal of normality of the reactions in comparison with the best physical and psychic conditions." Two fundamental types appeared from these tests: the type who slow up (exhaustion and weakness), and the opposite type (excitability and instability). A study of the individual reactions shows that the first type is characterized by a consistently long reaction time, 0.220 to 0.230 second with the visual stimulus; the second type presents a lower reaction time, 0.150 to 0.190 second, but an increased variability.

Emotional reactions were studied by the changes in circulation, respiration, and tremor of the hand, produced by stimuli designed to create an emotion. All the laboratories seem to have used the method described by Grandenigo and Gemelli (p. 45), in the selection of candidates for aviation, though the standards of classification are not reported further than to state that those who have an excessive reaction are pronounced ineligible. Of 13,936 candidates examined, 232 were rejected as "excessively emotional." The general behavior of the pulse and changes in blood pressure and volume and the duration of the changes were observed. The following were recorded: the pulse of the left fore-arm by means of an arm plethysmograph, the pulse of the carotid, thoracic breathing, the tremor of the right hand, and the time in seconds. The "emotive stimulus" was produced by a pistol shot, automobile claxon, or the explosion of a fire cracker.

Aggazzotti (p. 89) has reported more in detail the results obtained in the Rome laboratory, though he also fails to state how the tests were used in the selection of candidates. He, however, presents interesting tables of the results of the examination of 723 subjects. These are classified into ten groups according to their pulse rate at the moment of stimulus, ranging from 55–59 in the first group, to 140–149 in the tenth. Following the stimulus the reaction is divided into three phases: first, the effects of an inhibition of the vagus with an acceleration of the pulse; second, an excitement of the vagus with a slowing up of the pulse; and third, an excitement of the accelerator with a consequent acceleration. The first phase is brief (4–5 pulsations), and manifests itself almost immediately after the stimulus. Its latent period is negligible. The second phase is longer than the first (15–25 pulsations); it manifests itself rapidly and also ceases rapidly, to give place to the third phase. The third phase is the longest of the three; it appears rapidly and ceases slowly. These chronotropic reactions are always accompanied by inotropic reactions (strength of pulse). However, there is no relation in the degree of these two forms of the reaction, nor is there any relation noticed in the activity of the heart and the inotropic reactions at the moment of stimulus. The inotropic reaction appears in two phases: a negative phase of brief duration, three to four seconds, which appears in the first pulsations after the stimulus; and a positive phase of greater duration, which develops gradually after the first negative phase has ceased. The author does not discuss the relation between the degree of the various reactions and the pulse rate at the moment of stimulus, but an inspection of his table A (p. 91) indicates that as we go from those subjects with a low to those with a high initial pulse, the decrease in pulse rate in the second phase is more pronounced, but the increase in the third phase (acceleration) is less pronounced. The various reactions occur in more rapid succession with the more rapid initial pulse rates. The height of the systole also increases, but the proportional change with stimulus is not significant.

The influence of an emotive state upon an elementary process, such as reaction time, was studied by M. Camis (p. 188). The technique was similar to that employed in the other reaction time experiments, except that the "emotive stimulus" was introduced between two series of twenty or thirty readings with visual and auditory stimuli. In every case reported (twelve cases) the average is longer in the series following the emotive stimulus, ranging from 3.4 per cent to 30 per cent. These may be classified as "good" below 10 per cent, "medium" between 10 and 25 per cent, and "poor" or disqualifying above 25 per cent. The deviations from the average were also taken into consideration. These are presented in the form of curves of the individual reaction times plotted with reference to the abscissa representing the average. An inspection of these curves shows several types. The disturbance may be violent but fleeting, producing a few extremely long reaction times and quickly returning to normal, or the effect may be more moderate and lasting. A third type shows a fairly large deviation which is constant throughout the series, both before and after the emotive stimulus. The fourth and best type is characterized by a relatively small deviation in both series.

Short series of observations were made on the effects of the emotive stimulus upon choice reaction times. The results are uncertain. In the majority of cases the average of the second series is lower than that of the first, though the number of wrong reactions is greater. The author concludes that these facts lead him to believe "that a moderate emotional state, as is that induced by the methods of the laboratory, can frequently increase the velocity with which the psychic process develops. That the difference, that is, the increase in velocity, is to be ascribed to the superior psychic elements of this complex function, is demonstrated by the fact that in the psychomotor simple reaction, where the psychic functions, as judgment and choice, do not enter into play, one never has shortening, but only lengthening of the reaction times" (p. 196).

Attention was studied by various tests to determine the extent, distribution, concentration, fluctuation and intensity of attention, and the speed of perception.

The cancellation test devised by Saffiotti was used. This test consists of marking symbols arranged without any connection and placed irregularly so that the influence of the habit of reading is reduced to a minimum. Account is taken of the time required and the number of errors, the limits being set at five minutes and five errors.

Gemelli (p. 56) also used the successive exposure of small images at a rate sufficient for the subject barely to gather the details. The subject must react to the passing of each image and signal the passage of those images which presented the predetermined peculiarity. No results are given, but he states that the method "has proved excellent in the singling out individuals unfit from the point of view of the ability to concentrate attention for a determined time."

Another method of measuring attention was devised by Galeotti and Cacciapuoti (p. 207). Series of simple figures of different colors were exposed successively and the subject required to report immediately. The exposures were made by means of a kymograph at the rate of one figure every two and one half seconds, each figure remaining exposed one second. A record was kept of the number of figures reported and the errors regarding form, color and order. A figure omitted was counted as two errors, one for form and one for color. With four series of five figures each, it was found that the majority of subjects tested (338 subjects) made 13 to 16 errors; a minority, considered to possess insufficient attentive capacity, made over 20 errors. An examination of 36 candidates who had been declared ineligible because of "psychic nervous wear" demonstrated that 26 made over 20 errors. The limit of 20 errors was, therefore, considered a just standard of attentive capacity for rejection of candidates.

Both Herlitzka (p. 196) and Galeotti (p. 201) designed methods to test the speed of perception. Simple figures such as circles, squares and triangles were exposed by means of a falling shutter. The length of exposure was varied by adjusting the height of the fall or by adjusting the size of the opening. In this way the minimum time necessary to recognize the figures was determined. In Herlitzka's test a screen was lighted by two projectors so arranged that the falling shutter cut off the light of the first at the moment that it opened the second which projected the figures upon the screen. In like manner it reopened the first at the close of the exposure.

Both of these investigators obtained similar results, but disagree in their interpretation. Herlitzka believes that the individual variations in minimum time required for recognizing the figures indicate that the test must measure the psychic function. However, the time is too brief, 2–4.5 sigma, for such a mental process, and he is inclined to believe that after all his method merely determines the threshold or time of adequate stimulation of the retina. Particular evidence of this is cited in the fact reducing the distance between the subject and the screen from 2 meters to 80 cm. materially reduced the minimum time required. Galeotti, on the other hand, insists that their results are the measure of the speed of perception, though his evidence is not convincing. The fact that their methods were slightly different, but their results approximately the same, which his data does not entirely substantiate, and also the fact that he himself was unable to observe an after-image upon closing his eyes immediately after an exposure, are evidence to him that his test measures the psychic function and not merely the sensory threshold. It should be observed that the field was lighted only momentarily in his experiment. Herlitzka is not willing to recommend the test, in its present development, for the selection of aviators and Galeotti has apparently not so used it.

Perception of muscular effort was studied on the assumption that one of the important factors in the control of an airplane would be the recognition on the part of the pilot of the pressure exerted upon the controls and the amount of effort necessary to change their position a given amount. Apparatus was designed by Galeotti (p. 143) which required the subject, blindfolded, to hold a lever in a specified position while the weight suspended from the lever varied irregularly from 2 to 12 kgm. The movements of the lever were recorded upon a kymograph. The results are briefly summarized as follows: (1) Excellent curve, indicating excellent muscular aptitude. The curve remains in the middle horizontal zone of the tracing. The principal oscillations are uniform and low with few indentations and without jumps. (2) Mediocre curves, ascending and descending with not very high nor very uniform oscillations, but many indentations. (3) Poor curves, which indicate inaptitude rightly to regulate the muscular activity, those deformed with great indentations and jumps.

Gemelli (p. 58) also tested muscular effort, though he does not adequately describe his method. Apparently the subject was required to move a handle against a fixed resistance any required distance. The time required for the movement, and the accuracy of the judgment of the effort required, were taken as measures, not of his motor ability, but of his attention ability.

Equilibrium. In the selection of candidates for the air service the Italians, like the Americans, placed a great deal of emphasis upon the Barany tests. Grandenigo, president of the commission and inspector of the laboratories, is himself an otologist. The technique and standards of admission did not differ materially from those adopted in this country. However, Malan, an otology specialist, expresses the belief that while the Barany tests are necessary, they do not indicate the candidate's sensitivity to balance or change of position. "To possess an absolutely normal organism, to be immune from exaggerated manifestations of vertigo following rotation and transcephalic galvanization are certainly indispensable requisites to become a good aviator; however, besides all this, the so-called static sense of one's position and that of the vertical must not be lacking" (p. 159).

To test this sense of position, Malan arranged a special cockpit which could be tilted forward, backward or laterally. The subject, blindfolded, must report his position and at the same time place in the vertical position a rod in front of him attached to a goniometer. About 900 tests were made with 150 subjects. The results indicate that an inclination in the lateral direction is more easily recognized than in the forward or in the backward directions. Inclining the head forward at an angle of 30 degrees did not alter the results materially. The best results were given by pilots of long experience and in good physical condition. Sailors and a tight-rope walker also gave good results, especially in the lateral position. In aviators suffering from nervous exhaustion, as a rule the errors were much greater, but, examined after a complete rest, the errors were reduced to normal. No mention of the use of the test for classification of candidates is made. Individual differences and standards that might be established are also omitted.

Bilancioni and Romagna-Manoia (p. 175) present a preliminary study of the state of consciousness in rotary vertigo. The tests are rather crude and unsatisfactory. The subject, blindfolded, is required to recognize simple objects placed in his hand before and after turning with head resting on his knees. Naturally there is a good deal of disturbance due to compensatory movements as the subject attempts to sit up after rotation. However, when the object is placed in his hand the time for recognition is not appreciably longer. In a second series the subject was required to recognize simple figures exposed on a screen as soon as he could gain the upright position. This test also yielded negative results. The difficulties that would be encountered in such an experiment can be easily understood. As the experiments were performed it may be assumed that the tests were given after any effects upon consciousness had disappeared. If the tests had been applied immediately after rotation, the lack of motor control, particularly the tendency to eye movement would have vitiated the results.

It will be recognized that the Italian investigators attempted to use a great variety of tests and have undoubtedly acquired a great mass of data. It is to be hoped that definite correlations will be published, now that the war is over and opportunity for such a study is possible. It should be remarked that the disqualification of a candidate did not rest upon the absolute results of any test or group of tests. These served only to present the most definite profile it was possible to obtain. From these results compared with flying aptitude the following conclusions are presented:

1. Pursuit pilots present a keen perception of position of body, low visual reaction times, choice reaction times, and low average deviation in reaction time. Nearly all the best pursuit pilots were of the sensorial type in reaction time tests. Resistance to emotive stimuli was not necessarily great in all cases. Many of the best pilots in this group manifested low resistance to emotive stimuli.

2. Nearly all the best pilots on slow machines, such as the bombing planes, were of the muscular type in reaction time. Caproni pilots present considerable resistance to emotive stimuli.

FRENCH RESEARCH

The interests of the French seem to have been limited to simple reaction times and studies of emotional stability. A preliminary report of these experiments, "Recherches sur l'aptitude à l'aviation" by Camus and Nepper, is presented in *Bulletin de l'Institut Général Psychologique*, Nos. 1–3, 17^e année, 1917.

In the simple reaction time tests the D'Arsonval chronoscope was used. It was found that the average reaction times were 0.190 second with the visual stimulus, and 0.140 second with the auditory and tactual stimuli, but the limit of eligibility was set at 0.100 second above the averages. Only single illustrative cases are reported. Unfortunately, however, these cases seem rather conflicting. For example, one case is given to show that a single series of ten reactions is reliable as a measure of a candidate's reaction times, as it remains constant even after a year at the front where he was shot down by the artillery. The effect of morphine in one case was a slight decrease in reaction times in one examination, but four months later the test was repeated showing a decided increase. Still another pilot who had been shot down at the front is used to illustrate the effect in lengthening his reaction times. These are only isolated cases and it may be assumed that they were poorly chosen, for we learned that at the time of Nepper's death thousands of aviators and candidates had been tested by this method. It is to be hoped that a careful study of these results will soon be forthcoming.

The emotional study was identical in every way with the Italian work with the exception that these results were compared carefully with the results of the reaction time experiments. Both the average times and the deviations from the average were considered. On the basis of these two tests candidates were divided into five types:

1. Excellent pilots, who present normal reaction times and who do not show excitability.
2. Those who present normal reaction times but show a slight emotional reaction.
3. Those who present slightly irregular reaction times and show a slight emotional reaction. Usually young men who are apprehensive of the examination. These three types are considered acceptable for aviation.
4. Reaction times present large deviations and the emotional reactions are greatly exaggerated.
5. Reaction times are very irregular, though they do not show excessive emotional reactions. Types four and five are considered ineligible for aviation.

Since the report of Camus and Neppe was published, we learned, indirectly, that the use of the vaso-motor reaction test, as a means of determining the emotional fitness of candidates, was abandoned in favor of the regularity of the reaction times.

BRITISH RESEARCH

The British paid little attention to reaction times and the more elaborate studies of resistance to emotional stimuli. The latter is included in an incidental manner in a motor coördination test. The principal application of psychological tests by the British was in the determination of the effects of altitude flying and in studies of "staleness" in pilots, rather than in the routine examination of candidates. Their results are to be found in the Reports of the Air Medical Investigation Committee, published by the Medical Research Committee, London. The membership of the Air Medical Investigation Committee consisted of Henry Head, chairman, Sir Walter M. Fletcher, M. Greenwood, Leonard Hill, W. H. R. Rivers, C. S. Sherrington, C. E. Spearman, and Martin Flack. Most of the experiments are concerned with physiological data, pulse, blood pressure, vital capacity, etc. Some of these, however, present a psychological aspect. For example, the manometer tests involve a measure of volition, which has been recognized by Birley and others. The more distinctly psychological experiments included the MacDougall dotting test, studies of tremor and giddiness, and a study of temperament and service flying.

In the studies of the effects of altitude flying the British did not make use of a low-pressure chamber or other apparatus for diminishing the oxygen supply, though late in the war Lieut.-Col. George Dreyer had developed an apparatus that promised to have certain advantages over the rebreathing apparatus used in this country. The tests to determine the effects of altitude and the value of use of oxygen were made before and after flights. Flack and Heald (Report No. 1, "Oxygen Needs of Flying Officers," February 18, 1918) investigated the oxygen need at relatively low altitude by this method. The tests adopted were the pulse, blood pressure, the time the breath could be held, and the MacDougall test. The apparatus for the latter consists of a paper tape passed through a narrow slit by means of a clockwork. Upon the tape are small circles, each with a dot in the center. The circles are regularly spaced longitudinally but are irregularly placed laterally. The subject endeavors to spot the bull's eye of every circle with a pencil as the tape passes the opening. The rate of movement of the tape is gradually increased until a breakdown in the subject's reactions is secured. The tape is then scored according to the following rules: Ten complete misses out of twenty constitute a breakdown at the particular rate at which this occurs. The rate immediately preceding this breakdown is then assessed as follows: plus five for a bull's eye and minus one for a miss, twenty circles being counted. The fastest nominal rate is one, and for this 100 marks are given; for rate three, 80 marks; for rate five, 60 marks, and so on. A simpler method of marking was later adopted in which two marks were given for a bull's eye, one mark for touching the circle, and zero for a miss, twenty circles being counted at each speed. The results with the dotting machine indicate an improvement with administration of oxygen. Of nine cases, six deteriorated after flight without oxygen; after flight with oxygen all made improved performance, and in the cases where there was previously a deterioration, the previous performance was surpassed. Administration of oxygen to stale or mentally fatigued subjects increased their performance 30 to 40 per cent.

The British laid great stress on simple motor coördination tests, such as walking a line heel to toe and turning on one foot, standing on one foot for fifteen seconds with eyes opened or closed, balancing a rod on a flat board with eyes opened or closed, and on indications of tremor.

Walking along a line heel to toe, turning on one foot and standing on one leg with the eyes closed, was a test designed to determine "vestibular stability." But according to Head (Special Report Series, No. 28, "The Sense of Stability and Balance in the Air," 1919) "there is no evidence to show that any of these acts bears a direct relation to the activity of the semicircular canals," failure to pass the test is due to "clumsiness in action or slowness in comprehension." Likewise, the assumption that the ability to stand on one foot for fifteen seconds with the eyes closed was a test of "muscle sense" was criticised by Head who carried out tests to measure the power of recognizing passive movements of the lower extremities and found that individuals who were unable to stand on one foot with eyes closed did not lack the ability to recognize the passive movement of the lower extremities. Failure to pass the test is due to want of control and not to defective muscle sense. In a like manner he found that inability to balance a rod on a flat board was due, not to defective muscle sense as assumed, but to "want of control and clumsiness in action." The inability to balance the rod is not due to tremor, as several failed who showed no signs of tremor. The rod balancing test is recommended as a useful method of testing states of exhaustion, flying stress, insomnia and other neuropathic and psychopathic conditions in the early stages of development.

Giddiness and nausea in the air are ascribed to two groups of causes: (1) temporary abnormality of the middle ear, often due to a cold in the head and interference with the free passage of air through one or both of the Eustachian tubes, producing an abnormal condition of tension which acts on the vestibular apparatus, and (2) failure to learn adaptation to disturbances of equilibrium, which may be caused by lowered general resistance due to a physical or mental disturbance, such as influenza, or stress of flying at the front, "anything which tends to lessen control over the reaction of the lower centers to sensory stimulation." In every case investigated in the latter group, the subject had suffered in childhood from swinging and riding on the train. Most of them had suffered discomforts in the first flights but had become adapted to changes in equilibrium by repeated flights, though some never completely overcame some discomfort. Rotation produced no abnormal response, but was accompanied by excessive giddiness and nausea. It might be explained that the rotation test consisted, not in the time of nystagmus, but in the requirement that, after rotation, the subject should stand erect and walk forward. He at the same time reported the direction in which the room seemed to be turning. The degree of stagger and deviation in walking were considered the measure of his reaction.

Fainting in the air, on the other hand, was never accompanied by any discomfort. None of the subjects examined had ever experienced any discomfort in flying. They seemed to possess the adaptability or power to control the effects produced on the vestibular apparatus by changes of equilibrium. "But when the impulses of the semicircular canals became too violent to be held in check, they expended themselves by producing a withdrawal of consciousness." Fainting is described as pleasant; it begins with a narrowing of the field of attention and may culminate in complete oblivion. The reaction upon rotation of these pilots was characteristic. The effect upon those who were giddy in the air was, when rotated, excessive reaction in the form of giddiness and nausea, though their ability to exercise voluntary control was normal. On the other hand, of five who had fainted in the air, three suffered no discomfort of any kind when rotated; in two cases "everything went misty," and both reported they felt exactly as they did in the air.

The presence and degree of tremor, revealed by requiring the subject to stretch out his arms in front of him, fingers semiflexed and separated, and eyes closed, at the same time protruding his tongue, was carefully noted. According to Flack and Bowdler (Report No. 2, III, "Report on the Examination of a Series of Successful Pilots from the Point of View of their Cardio-vascular and Nervous Systems," March 23, 1918) a high correlation is found in the group which showed tremor with want of aptitude to fly. Tremor is absent or slight in the good pilot. Out of 54 candidates who were accepted in spite of "marked tremor," 14 did not qualify, a ratio of success to failure of 2.8 to 1. Amongst 2000 candidates who showed no abnormal signs, 7.6 per cent only were ultimately rejected. It is the conclusion of Head (op. cit.) that "tremor is not so much an indication of a neuropathic temperament as of a definite disturbance of function." A tremulous tongue may be indicative of chronic indigestion or of alcoholic excess. Tremor of the hands was always found to be associated with some obvious cause, as alcoholic excess, excessive smoking, especially the perpetual misuse of cigarettes, or as a sequel of malarial infection. It was found most commonly, however, as a sign of stress of service at the front, exhaustion, and states of anxiety, and was universally present in those officers who were admitted to hospital for some functional psychosis ("shell shock"). The "line drawing and noise test" was regarded as a valuable test and its use recommended in the examination of candidates and pilots. An account of the test was furnished by correspondence by Professor Spearman:

> The candidate is asked to draw a line lightly and deliberately across the greater length of a sheet of foolscap, parallel to the border, with a leaded pencil, without the hand or arm touching the paper. The pres-

ence and degree of tremor can be seen, especially if a magnifying glass is used. He is then asked to draw another line in the same way; while he is doing so, an unexpected loud noise is made close to the ear with a Dalby's "clacker," which makes a loud noise and is easily manipulated and hidden. A perfectly sound man will often continue the line without interruption, or a slight irregularity will appear in the line before it is again firmly continued. The effect on a nervous man is either to produce a sudden dash and complete stop, or the line is continued in an increasingly irregular and shaky manner. He is then asked to draw a third line, after being warned that the noise will be made at some point. If the nervousness is under control, the line will not be interrupted; if not, the same hurried dash and complete stop is again seen or the rest of the line is drawn with increased tremor and irregularity.

Lieut.-Col. J. L. Birley (Report No. 4, "Temperament and Service Flying," September 25, 1918) has made the attempt to determine the influence of "temperament" upon the ability not merely to fly but to do effective combat flying at the front. His method is that of the elimination of the possibilities of physical causes of "staleness" by means of the regular physical examination. He makes no attempt to analyse temperament or determine the special temperamental qualifications for service flying. From a study of the results of the manometer tests (expiration force and time of sustaining a 40 mm. column of mercury by blowing), blood pressure, and pulse rates, from pilots and observers transferred to England for rest from active service at the front on account of fatigue, he concludes that "temperamental and physical unfitness are not separate but closely allied things," and that "the temperamentally unfit is unstable physically." "Certain individuals are so constituted that they react to strain by alteration of their physical state, or in other words that by reason of their temperament they will necessarily become physically unfit when exposed to strain. This lack of resistance appears to be a characteristic of the general 'make up' of the individual, and it must be supposed that the lack of inhibition in the higher nervous centers paves the way for a spread of nervous impulses; this in turn gives rise to a variety of physical phenomena, the main feature of which is overaction," which manifests itself in the somatic system in various neuro-muscular groups, more especially in the speech mechanism, in the autonomic system in rapid and disordered heart action, sweating, hyperadicity, raised blood pressure, and loss of weight due to general increase in metabolism.

AMERICAN RESEARCH OVERSEAS

Several papers concerning psychological research in aviation in this country have appeared in the American journals and need no further mention. Some of the published reports have been attempts merely to demonstrate a method, others have involved a careful attempt to determine the degree of correlation of the results with flying ability (1). The results of the various lines of research carried on by the psychology department of the Air Service Medical Research Laboratory are also about to be published.

The work of the Air Service Medical Research Laboratory in the American Expeditionary Forces, before the signing of the armistice, consisted principally in routine examinations of student flyers as conducted at the various fields in this country. As the number of these examinations was small compared to the number at home and as no new or significant data was presented, a discussion of these results will be omitted in lieu of the forthcoming reports from the home laboratory. After the signing of the armistice the officers of the research laboratories at Issoudun and Tours undertook a study, as well as conditions permitted, of those flyers who had made excellent records at the front and in the instruction centers. Three groups of flying officers were examined, the moniteurs of the instruction center at Issoudun, chasse pilots from the front who returned to Issoudun on their way home, and observers from the front who returned to Tours. It should be stated that only those pilots who had proved to be exceptional service flyers were examined. The moniteurs examined were declared by the commanding officer of the field to have been selected and retained at the instruction center because they were recognized by the training department as possessing all the qualifications of good service flyers and would doubtless have distinguished themselves at the front, had they not been detained for instruction purposes.

All of these subjects were first examined by the departments of medicine, physiology, ophthalmology, and otology. Those diagnosed as unfit, because of abnormal pulse, blood pressure, respiration, or any other specific reason that might influence their results in the psychological tests have been considered separately. In addition several who showed unmistakable signs of suffering from exhaustion, nervousness or recent excitement that might render their results unreliable were also placed in the group of unfit.

The psychological tests applied to the moniteurs and chasse pilots consisted of the standard altitude test, or rebreather, simple reaction times to visual, auditory, and tactual stimuli, and a steadiness test. With the observers at Tours, in addition to the altitude test, a new test of speed, reliability and confidence of observation was adopted.

Rebreather test. While the purpose of the rebreathing test is primarily for the altitude classification of the flyer determined by his resistance to effects of low oxygen, it incidentally reveals certain individual psychological characteristics, involving as it does the performance of a task requiring coördinated, rapid movements, and alert, extensive attention, the deterioration of which is carefully noted during the oxygen diminution. An intensive study of this feature of the rebreathing test on 5000 pilots examined in the United States is being prepared by Dr. J. E. Coover, formerly captain, Sanitary Corps, on duty with the Medical Research Laboratory, and a comparison with the results in the American Expeditionary Forces will be made upon its completion. This comparison is significant because the group in the American Expeditionary Forces was composed of advanced pilots, from which all the inferior or unsuccessful flyers had been eliminated, while the group in the States contained many who were still in training and subsequently proved unfit for flying. A preliminary examination of the data seems to indicate that with the pilots in the American Expeditionary Forces there is a more decided tendency to maintain effective attention longer after the appearance of disturbance of motor coördination, than is the case with pilots examined in the States.

Reaction time tests. As the French depended upon simple reaction times solely in their psychological examination of candidates for aviation, it was recommended by the Medical Research Board that the psychology department investigate this type of test as to its validity. The moniteurs and chasse pilots afforded good material for this investigation, as they were all men of proved ability. In addition we were able to secure the rating of a large number of these men from the training department of the instruction center at Issoudun. The degree of correlation between their reaction times and the training department rating should indicate the value of the tests.

The attempt was made to reproduce as nearly as possible the experimental conditions of the French methods as developed by Dr. Nepper. The reaction times were recorded with the D'Arsonval chronoscope used by Nepper, with the exception that part of the reactions to the auditory stimulus was recorded with the Hopkins chronoscope in 0.001 second. The D'Arsonval chronoscope was read in 0.005 second. The tactual stimulus was given with a touch key, which was also used for giving the auditory stimulus. The visual stimulus was the starting of the hand on the dial of the chronoscope. Though Nepper recorded only ten reactions with each stimulus, claiming that this number gave as reliable a mean as a larger number, we made it a rule to record twenty-five. The results of our experiments do not tend to confirm his view, though for clinical purposes the variation with each succeeding ten readings is not so great as to be very important.

Table 1 gives the results of the moniteurs and chasse pilots combined, showing the mean (M) in sigma, probable error (PE) and the standard deviation of the mean (σ) for the entire group based on the mean reaction times of each individual tested, and also the mean, probable error and standard deviation of the coefficients of variation of each individual from his mean reaction time.

The corresponding mean reaction times reported by the French (Camus and Nepper, op. cit.) are 0.190 second for the visual stimulus, and 0.140 second for the auditory and tactual stimuli. They do not report definitely the amount of deviation from these means, except to say that more than 0.10 second lengthening in reaction time was considered sufficient to bar a candidate. They also apparently considered 0.10 second as the maximum deviation of an individual from his own mean reaction time. It will be observed that our means are somewhat higher than the French and that there is a wider deviation not only between individuals, but also in the separate reaction times of each individual.

TABLE 1

	NUMBER OF CASES	REACTION TIME			COEFFICIENT OF VARIATION		
		M	PE	σ	M	PE	σ
Visual	148	197	±0.99	17.9	8.3	±0.17	3.0
Auditory	192	155	±0.95	19.5	11.4	±0.16	3.3
Tactual	90	143	±1.76	24.7	13.4	±0.23	3.5

TABLE 2

	VISUAL REACTION TIME			AUDITORY REACTION TIME		
	M	PE	σ	M	PE	σ
Moniteurs	189	±2.98	23	153	±1.54	19
Chasse, fit	197	±1.15	17	157	±1.24	18
Chasse, unfit	205	±4.22	22	158	±2.73	20

In table 2 we have separated the results of moniteurs and chasse pilots. The chasse pilots are further divided into the "fit" and the "unfit" on the basis already explained. All of the moniteurs were reported physically fit. Reactions to tactual stimulus are omitted from this table owing to the few moniteurs and unfit chasse pilots that are included.

A further check upon the value of reaction times in the selection of aviators is the comparison with ratings in flying ability furnished by the training department. We should not expect too high a correlation, even though the test is a good measure of aptitude in flying, for doubtless the training department rating is frequently influenced by other factors, not actually related to flying ability. Strict observance of military forms, sociability, etc., can not be strictly eliminated from a rating of this sort made by several different officers under diverse conditions. We might expect, therefore, that a man might be misplaced one or more classes in either direction due to these irrelevant factors.

It was desired for the convenience in handling the data that the men be classified into ten groups according to general flying ability. Captain Street, in charge of the training department, using all the record available in his office, performed this work for us. None of the men we examined fell in the last four groups, so there are practically six grades classified. The results of correlation of these ratings with reaction times and the coefficients of variation are given in table 3. The coefficient of variation is given, as it may be an index of stability or emotional control, as Nepper suggested. The Pearson correlation formula (Davenport, C. B.: Statistical Methods, p. 45) was used:

TABLE 3

Correlation of training department rating with average reaction times (R.T.) and coefficients of variation (C)

	NUMBER OF CASES	R. T.		C.	
		r	PE$_r$	r	PE$_r$
Visual reaction times					
Total......................	110	0.051	±0.064	0.098	±0.053
Fit........................	93	0.113	±0.069	0.073	±0.069
Auditory reaction times					
Total......................	148	0.095	±0.058	0.042	±0.055
Fit........................	132	0.132	±0.058	0.188	±0.057
Tactual reaction times					
Total......................	68	0.075	±0.082	0.011	±0.083
Fit........................	60	0.037	±0.087	−0.018	±0.088

$$r = \left(\frac{\Sigma(x'y')}{n} - \nu_1'\nu_1''\right)\frac{1}{\sigma_1\sigma_2}$$

$$PE_r = \frac{0.6745\,(1 - r^2)}{\sqrt{n}}$$

All the correlation values with reaction times are low, but the fact that they tend in the same direction may have some significance, noting that correlations with visual and auditory reactions are somewhat higher when the unfit are eliminated. It is very doubtful, in our opinion, however, that the simple reaction time can be regarded as an adequate test in the selection of aviation candidates. It is possible that more complex reaction experiments, such as those that involve discrimination of varying degrees of difficulty, judgment and other modes of association, may yield important results. Such experiments in the American Expeditionary Forces, with the limited equipment and time at our disposal, could not be carried out satisfactorily. Experiments in choice reactions were attempted, but the results are too meager to be of value.

Steadiness test. The apparatus for the steadiness test consisted of a sheet of copper about one thirty-second of an inch thick and six inches square in which holes were drilled $\frac{12}{64}$, $\frac{14}{64}$, $\frac{18}{64}$, and $\frac{20}{64}$ of an inch in diameter. This plate was attached by one corner to the window sash and could be turned up or down sufficiently to bring any hole on a level with the eyes of the subject when he stood erect. It was connected in series with a dry cell, stylus, and signal magnet, adjusted to mark upon the smoked paper of a kymograph revolving once in a little over one minute. The subject was instructed to stand erect with the right arm free from the body, but in as easy a position as possible, and with the stylus held horizontally between the thumb and fingers as he would held a pencil in writing. He was to take his time in inserting the stylus into the hole and was further informed that no record would be made until he had fairly done so. He was to hold it in the hole without touching the sides for a full minute. He was warned against holding his breath, as this would interfere with his steadiness before the end of the minute, and quite regular breathing was recommended. The largest hole was used first. After the subject had rested about half a minute the next smaller hole was used, and the other two holes in the same manner.

Unfortunately the fixing solution used to preserve the records was unsatisfactory and the records were so badly blurred before we could count the number of touches that it was deemed advisable to give up the attempt of such a quantitative study. Accordingly the subjects, 128 in number, were divided into five groups on the basis of the appearance of tremor as shown by inspection of the records for the smallest hole. Group 1 (table 4) includes those subjects whose records showed only a few scattered contacts; group 5, those showing practically continuous tremor.

TABLE 4

GROUP	FREQUENCY
1	16
2	34
3	38
4	19
5	21

TABLE 5

RATING	FREQUENCY	
	Group 1	Group 5
1	1	0
2	5	2
3	9	3
4	2	5
5	2	7
6	0	0
7	0	0
8	0	1

The best nine in group 2 were selected and added to group 1, and the poorest four in group 4 were added to group 5, making twenty-five in each of the extreme groups. Of these it was found that the training department could furnish ratings in aviation ability on nineteen in group 1, and on eighteen in group 5. The distribution of these ratings in the two groups is shown in table 5.

To determine the relationship between the training department rating and tremor, the Pearson cos π method, in a fourfold table (2) was used:

$$r = \cos \frac{\sqrt{bc}}{\sqrt{ad} + \sqrt{bc}} \pi \qquad PE_r = \frac{1.1}{\sqrt{n}}$$

in which

a =	number of cases no tremor (group 1) and high rating (1, 2, 3).	15
b =	number of cases, marked tremor (group 5) and high rating.....	5
c =	number of cases, no tremor and low rating (4, 5, etc.)..........	4
d =	number of cases, marked tremor and low rating................	13

The index of correlation was found to be 0.725±0.181, in very striking agreement with Head's statements regarding the British findings in their studies of tremor.

Observers test. Very little attention has been paid to the selection of men best qualified for aerial observation, and it was strongly impressed upon the officers of the Medical Research Laboratory at the Observers School near Tours, to which observers from the front were sent on their way home, that the aerial observer, from the special demands made upon him and the importance of his work, must be as carefully selected and as highly trained as the pilot.

Of the various types of work required of the observer, infantry and artillery liaison, bombing, photographic missions, sketching, and visual reconnaisance, the last is of especial interest to the psychologist. The observer, on a visual reconnaisance mission, must know his sector perfectly from a topographical point of view, and must observe and report to the intelligence officer any changes in the enemy troops and organization, and in the enemy troop rest area, going to a depth of 50 kilometers behind the enemy lines. What is called "cloud reconnaisance," a type of observation recommended, gives a suggestion of the intelligence and keenness required of the observer. Very often the clouds are low and form a solid bank at probably 1000 or 2000 meters. The enemy takes advantage of the weather to move troops or convoys that they would not attempt on clear days. To detect such movements the observer climbs up through the clouds, flies by compass until he arrives at the point he wishes to observe, dives down through the clouds, gets all the information that he can, climbs up through the clouds again, and goes to another point in the same manner. When the observer dives through the clouds, he must, of course, get his bearings and orient himself instantly, besides making accurate and detailed observations. Among the observations that he must make and report on are: (1) Movements along roads, behind the enemy lines, of troops, their location and direction, and an estimate of their number; likewise of transports, their nature, whether general service wagons (rations, forage, supplies, etc.) or guns, and whether motor or horse drawn. Also movements along railroads, whether normal or abnormal; trains, whether standard or narrowgauge, their location and direction, and number of cars in the train; the nature of the train, whether trucks or carriages, covered or uncovered, loaded or empty. (2) Dumps, material and ammunition, and the rolling stock at stations and in the yards. By noting increase or decrease in the size of the dumps, and the concentration at railroad stations, it is possible to determine the intentions of the enemy in that particular area. (3) Airdromes, increase in number and their location, whether occupied or unoccupied, and the activity as indicated by the number of planes in the field. (4) Camps, hutments, and bivouacs must be watched constantly for troop movements, and new points of concentration by noting tracks, paths, smoke from kitchens and bonfires as signs of activity. (5) Villages and towns located behind the enemy lines, noting such things as the number of people loitering about the streets, to determine to what extent they are being used to station troops. (6) New lines of defense, battery positions, especially before their camouflage is completed, trenches, gun emplacements, railroads under construction, must all be watched very closely. (7) Enemy aerial activity, the number of planes, their type, insignia and markings, location and altitude, and their aggressiveness; hostile balloons, and the exact position of the balloon beds; and antiaircraft artillery, their activity, which is usually a good indication of the enemy's intentions.

With these demands on the observer in mind, it was desired to devise a laboratory test that might reveal individual differences in the quickness, accuracy and certainty of the observation of a situation, requiring definite study, especially of the nature and relation of parts, in its recall, and in its recognition when again presented. The test here described was applied to about fifty observers returned from the front. Incidently the introspection many volunteered seemed to indicate that it involved a very similar mental performance to that in actual conditions of visual reconnaisance in aerial observation. A series of aluminium slides, 16 cm. square, each ruled off in 2 cm. squares, and with holes, 5 mm. in diameter, drilled at six or seven of the forty-nine cross-lines, was prepared. These holes were covered with red, green or white tissue paper or gelatin. When a slide was inserted in the window of a light-box and illuminated from behind, the holes appeared as colored lighted discs of about equal brightness on a black surface defined by a white border. The window was covered by a thin open weave black cloth, and protected from side lights by a hood 14 cm. deep, and when unlighted the surface of the slide appeared as a uniform black area. The light-box, on a swivel base so that it could be swung aroung to the experimenter for changing slides outside of the subject's view, was placed on the table at which the subject sat, about 1 meter in front of him. A slide was inserted in the window, illuminated, and the subject given instruction and practice in plotting the arrangement, by noting their relation to the border and to each other, and indicating the color, of the discs, on a piece of coördinate paper ruled in 1 cm. squares. When the procedure was learned, the light was cut off, the practice slide removed and replaced by another. The subject had at hand a telegraph key by which he could at will light the box, and, by holding the key closed, keep it lighted as long as he desired. He was instructed to turn on the light and study the arrangement of the colored discs until he felt certain he could map them accurately on the coördinate paper, and to do this in the shortest necessary time. The experimenter, at the side where he could watch the work, noted on a stopwatch (1) the *time to study* taken. When the subject indicated his readiness to map the slide by releasing the key and darkening the box, he was given pencil and coördinate paper and (2) the *time to report* the arrangement and indicate the colors was noted. The subject was then asked to say how certain he was of the accuracy of his report. "Certain that it is accurate," "Doubtful if accurate" (indicating doubtful points by a question mark), and "Certain that it is not correct," noted by the experimenter under (3) *confidence of report* as A, B, and C, respectively. The experimenter noted (4) the *accuracy of report* and if incorrect, of if confidence B or C, another trial was given following the same procedure, and (5) the *number of trials* necessary for accuracy and confidence A noted. When this part of the test was completed, the slide was removed, and a dozen slides, among which several times at random this slide which had just been studied and mapped appeared, were inserted in succession, illuminated by the experimenter, and the subject instructed to react by closing one of two keys as quickly as he identified each as the "same" or "different" from the one he had learned. The time elapsing between the illumination of the slide by the experimenter and the discrimination recognition reaction by the subject was measured in 0.01 seconds by a time marker on a kymograph.

In scoring the results, the accuracy was graded on the basis of 100 for correct reproduction, debiting 8⅓ points for each misplaced disc, 8⅓ points for each wrong indication of color, and 16⅔ points for each disc added over the correct number six, which was the number on the slide. The value of confidence was graded by assigning 100 for correct report and judgment by the subject that he was certain of his accuracy (A), or if incorrect and judged as certain that it was inaccurate (C); assigning 50 for both correct and incorrect report where the judgment made was that he was uncertain as to his accuracy (B); and assigning 0 where the judgment was made as certainly correct or certainly incorrect when the accuracy was the opposite of the judgment. Where several trials were necessary before a correct report with confidence A was made, the total time to study, the total time to report, the average accuracy, and the average value of confidence, were calculated. The average results of the group of 47 observers returned from service on the front are given in table 6.

In the discriminative recognition reaction times, comparison was made between the reaction times of the sixth and seventh slides in the series, the sixth being "Different" and the seventh being "Same," having been preceded in the order: first Different, second Same, third Different, fourth Different, fifth Same, the Different slides in every case being different from each other as well. Forty-one observers returned from the front gave the average results shown in table 7.

TABLE 6

	M	PE_M	σ	RANGE
Number of trials necessary	3.45	±0.19	1.93	1–8
Total time to study, seconds	102.45	±6.02	61.20	27.0–319.0
Total time to report, seconds	149.04	±9.33	97.56	18.0–434.0
Average accuracy, per cent	80.96	±1.25	12.68	100.0–50.0
Average value of confidence, per cent.	65.00	±2.14	21.75	100.0–17.0

TABLE 7

Reaction times in 0.01 second

	M	PE_M	σ	C	RANGE
"Different"	97.92	±3.80	36.10	0.37	46.0–227.0
"Same"	92.55	±3.30	31.35	0.34	34.0–159.0

The difference between the averages of the two reaction times is slight and not significant ($D = 5.37$, $PE_D = \pm 5.03$) although the variability of the "Different" reactions is slightly more, and the range greater, than that of the "Same."

An attempt was made to determine the relation of the performance in this test to success as an observer. Ratings 1, 2, and 3 (1 being the best), were made of these observers by Capt. Cary, M. C., at an interview which he held with each man, basing his estimate of the man's ability on a complex of impressions gained from his appearance, manner, attitude, report of his experience at the front, etc. Unfortunately, no more objective measure of the observer's success or failure at the front, and specifically as an observer on visual reconnaisance, could be obtained. For purposes of adequately testing the test, such a rating would, of course, be necessary. Comparison between two groups, group 1 containing those rated 1, and group 2, those rated 2 and 3, are shown in tables 8 and 9.

Inspection of table 8 does not seem to differentiate the two groups in any striking way. Differences in the means are present but not significant. The difference (D) between two means to be significant should exceed 4½ times the value of the probable error of the difference (PE_D). In one case only we find a significant difference, that in Average Accuracy, in favor of group 2. As to variability (C), group 2 is more variable in "total time to study;" while group 1 is more variable in "average value of confidence." In the case of "average accuracy," while the difference in means is in favor of group 2, the variability of group 1

TABLE 8

	GROUP 1. 24 CASES. CONTAINING RATING 1				GROUP 2. 23 CASES. CONTAINING RATINGS 2 AND 3				GROUP 1–GROUP 2	
	M	PE_M	σ	C	M	PE_M	σ	C	D	PE_D
Number of trials necessary......	3.75	±0.27	1.99	0.53	3.13	±0.26	1.84	0.59	0.62	±0.38
Total time to study, seconds........	108.75	±7.05	51.20	0.47	95.85	±9.51	67.80	0.71	12.90	±11.84
Average time to study, seconds.....	34.60	±2.29	16.17	0.48	37.20	±2.89	20.60	0.55	− 2.60	±3.68
Total time to report, seconds........	162.51	±14.26	103.83	0.63	135.00	±12.42	88.50	0.66	27.51	±19.12
Average accuracy, per cent........	75.83	±1.82	13.20	0.17	86.30	±1.33	9.46	0.11	−11.30	±2.25
Average value of confidence, per cent........	59.17	±3.39	24.42	0.41	71.09	±2.39	16.33	0.23	−11.92	±4.15

is greater, and the coefficient of correlation between rating and accuracy in the total group was found to be high, 0.482±0.172 (Pearson's co π method).

Comparison of the two groups in discriminative recognition time is shown in table 9.

It is seen that group 1 is decidedly quicker than group 2 in recognizing the slide that has been studied and mapped ($D = 34.30$, $PE_D = \pm 6.58$). The coefficient of correlation was found to be high between rating and quickness in this recognition, 0.729±0.173. This fact is interesting when it is recalled how important such recognition is in conditions of actual aerial observation. Group 1 is quicker (18.5 per cent) in its identifying the "Same" than the "Different," while group 2 is slower (11.2 per cent), the difference in the means being for group 1, 17.00±7.10, for group 2, −10.98±6.94.

TABLE 9

Reaction time in 0.01 second

	GROUP 1. 20 CASES. CONTAINING RATING 1				GROUP 2. 21 CASES. CONTAINING RATINGS 2 AND 3				GROUP 1–GROUP 2	
	M	PE_M	σ	C	M	PE_M	σ	C	D	PE_D
"Different"........	92.00	±5.84	38.78	0.42	98.32	±4.61	31.40	0.32	− 6.32	±7.42
"Same"...........	75.00	±4.04	26.84	0.36	109.30	±5.19	35.30	0.32	−34.30	±6.58

Personality. Aside from laboratory test methods of discovering the qualifications that make the successful flyer, the Medical Research Laboratory sought to get an insight into the temperament and personality of these men by systematic interviews, and more particularly in the arrangement made by which one of the writers (Captain Dockeray) completely identified himself with the flying personnel, taking the R. M. A. training, and being put on flying status. This officer, when assigned to flying duty, ate at the flying officers' mess, reported at formations, and in every way possible conformed to the routine of flying training that a student flying officer undergoes. Thus in addition to the experience in flying, he was able to meet more intimately hundreds of flying officers, who talked much more freely than when they came to the laboratory. In this way the personality of the aviators, their habits of recreation, manifestations of staleness, and their flying aptitude as observed on the field were studied. The general impression gained was that flying demands and encourages a degree of individualism unknown in any other branch of the army, something which has been given full recognition in the French, and especially in the British service, but too often not in that of the United States. In learning to fly, as in the acquisition of any complex act of skill, there are definite periods of lapse in improvement due to certain difficulties in adjustment of the organism, and also to the change in attitude of the learner. The former should be understood by the moniteurs and others directly in charge of the cadet's training, as it would be of assistance in preventing accidents, as well as in judging the cadet's ability. The attitude of the cadet is largely influenced by his success, but also by the physical conditions surrounding him, such as quality of food, sleep, recreation, and the relation to his superiors. There are times when the aviator does not "feel like flying," as he says, not due to lack of willingness or desire to fly, but due to an inability to "pull himself together." It is a common belief among aviators that they should not be compelled to fly at such times. There is no doubt that often this feeling can be overcome, especially during emergency, but it is equally certain that it should always receive due consideration during the first weeks of training, and should be thoroughly understood by those in charge of flying.

As to the personality of the aviator, it seems that no general rule can be laid down. Quiet, methodical men were among the best flyers. What seems most needed by the aviator is intelligence, that is, the power of quick adjustment to a new situation and good judgment. He need not be so quick in *motor* adjustments, provided he thinks clearly or makes quick *mental* adjustments. The nervous, high strung individuals, or those bordering on the temperamental, are the least reliable, for though they often become good flyers, they are the most liable to become psychotic under stress.

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THE PHOTOTROPISM OF LAND SNAILS[1]

G. C. WHEELER

It does not seem probable that an organ as well developed as the eye of a gastropod should be functionless. Yet Yung (1911, 1913) claimed that *Helix pomatia* L. is totally blind and quite indifferent to light, notwithstanding the fact that Willem (1892) had previously given reasons for supposing that this species is positively phototropic. Hence I have undertaken to determine whether the eyes of *Helix* are functional or not. The problem was suggested to me by Professor G. H. Parker and the experiments were carried on under his immediate supervision. I wish here to express my thanks for his valuable advice and helpful criticisms.

The animal used in the experiments was *Helix aspersa* Müll., an introduced species which has become established in Georgia. Each snail was isolated in a small battery jar and was given a number in order that individual records might be kept. The bottom of the jar was covered with a layer of earth, which was kept moist. The animals were kept in a dark room at a temperature of about 21°C. Apples constituted the main food; fresh cabbage leaves were also used.

The apparatus in which the tests were made consisted of a wooden box divided by a partition into two compartments of unequal size. The smaller compartment, which contained a 12-candle-power electric lamp (*L*), was provided with a ventilated cover and rendered light-proof. A large rectangular opening was cut in the partition. This opening was covered with a diaphragm of black cardboard in the center of which was a circular aperture (*A*) 2.5 cm. in diameter. The center of this opening was placed on the level of the middle of the filaments of the lamp. A screen of thin white paper (X) was inserted between the lamp and the diaphragm to avoid unevenness of illumination due to the filaments. Since the experiments were carried on in a dark room, the larger compartment was left uncovered. In it was placed horizontally a circular piece of ground glass (G) 32 cm. in diameter. By means of wooden blocks this plate was raised to the level of the bottom of the aperture in the diaphragm. The inner walls of both compartments were coated with dead-black paint.

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[1] Contributions from the Zoölogical Laboratory of the Museum of Comparative Zoölogy at Harvard College. No. 328.

When it was desired to start a snail to creeping, the animal was subjected to a more or less prolonged immersion in water. Then it was picked up by the shell and placed on the plate in the large compartment (see S) 5 cm. from the diaphragm (20 cm. from the lamp) and immediately in front of the aperture so that the long axis of its body was perpendicular to the light rays. This method of handling did not cause an actively creeping snail to retract even its tentacles. The plate was kept moist and was washed frequently to free it of slime.

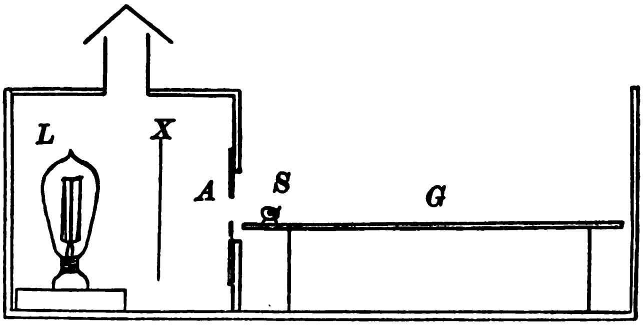

FIG. 1. DIAGRAM OF THE LIGHT APPARATUS IN WHICH THE SNAILS WERE TESTED

A, aperture between the two compartments in the box; *G*, ground-glass plate on which the snails were tested; *L*, electric lamp; *S*, snail; *X*, screen of white paper.

If the snail, when placed in the position just described, turned toward the light, the reaction was considered positive. If it turned away from the light, the reaction was regarded as negative. If it turned neither in one direction or the other, but moved straight forward, the response was regarded as indifferent. An animal was also considered indifferent when it turned alternately to the right and left. When one reaction was completed, the animal was lifted up by the shell and turned around so that the light was received on the opposite side of its body.

Ten snails were used in the experiments and an individual record was kept for each. Eight individuals were found to be negatively phototropic, one was indifferent, and one was too sluggish for experimental work. The reactions of any given animal were fairly constant from day to day, as is shown in table 1.

TABLE 1

Locomotor movements, negative, indifferent, and positive, of snail no. 2 on different days

	DECEMBER 5	DECEMBER 10	DECEMBER 11	DECEMBER 12	DECEMBER 18	DECEMBER 19	TOTALS
Negative..........	26	7	12	9	14	18	86
Indifferent........	0	7	0	0	4	3	14
Positive...........	4	2	2	0	1	2	11

To ascertain whether the negative phototropism of *Helix aspersa* was dependent upon the eyes or not, snails were tested for their responses to light, after which the eyes were amputated and the tests repeated. Amputation was easily accomplished with a fine pair of scissors when the tentacles were fully extended. The success of the operation could be determined by the presence of the amputated eye on the scissors. There was no evidence of shock subsequent to the loss of the organs; on the following day it was observed that the snail had eaten as much as a normal individual and was quite as active as the latter. The results of these tests are well illustrated by snail no. 4, whose records are given in table 2.

It is quite evident that the effect of amputation was to render the snail indifferent to light—conclusive proof, when taken in connection with the previous tests, that the eyes are functional. This experiment also shows that the skin of the snail is not sensitive to light, that is, the animals are not dermatoptic.

The experiment of cutting off only one eye at a time was also tried. As a result the snail became indifferent when the light was thrown on the eyeless side, but continued negative to light on the opposite side. The result was the same for the right eye as for the left. This seems to indicate that the organ is sensitive only to light falling into it from the side. As was to be expected, when the second eye was removed, the animal became wholly indifferent. The responses of snail no. 5, given in table 3, illustrate these tests.

TABLE 2

Locomotor movements, negative, indifferent, and positive to light, of snail no. 4, with and without eyes. The eyes were amputated after the tests of December 10

	WITH EYES			WITHOUT EYES							
	December 5	December 10	Totals	December 11	December 12	December 17	December 18	December 23	December 26	January 6	Totals
Negative	20	9	29	3	1	1	3	3	7	2	20
Indifferent	0	0	0	2	10	10	14	13	14	10	73
Positive	10	1	11	3	1	1	1	1	10	1	18

TABLE 3

Locomotor movements, negative, indifferent, and positive to light, of snail no. 5. Tests were made first in the normal animal, then after the right eye had been amputated, and finally after the left eye had been amputated. After the removal of the right eye the snail was illuminated first on the right side and then on the left, and the same procedure was followed after the removal of the left eye

	WITH BOTH EYES		ONLY LEFT EYE FUNCTIONAL				WITHOUT EYES	
	December 26	December 30	January 7		January 14		January 15	
			Side illuminated		Side illuminated		Side illuminated	
			Right	Left	Right	Left	Right	Left
Negative	16	22	2	9	1	4	0	1
Indifferent	5	3	9	2	1	0	4	3
Positive	2	1	1	1	2	0	1	1

To determine whether the heat from the lamp had any effect on the reactions of the snails, temperature tests were made. A centigrade thermometer graduated to tenths of a degree was suspended in the large compartment so that its bulb was in the same position as that occupied by the snail in the previous experiments, but without allowing it to touch the plate. The bulb was painted black in order that it might absorb a maximum amount of heat. All light from the small compartment was excluded from the larger one by placing a black shutter over the diaphragm. The larger compartment was illuminated by a weak diffuse light from above. After ten minutes the thermometer was read. Then the shutter was removed and the light from the lamp was allowed to fall on the bulb of the thermometer for ten minutes. No change of temperature was observed.

The lamp was replaced by a brass tube one-fourth of an inch in diameter, through which live steam was passed. After ten minutes with the shutter over the diaphragm, the thermometer was read. Then the shutter was removed and after ten minutes a second reading was made. The temperature had risen 0.2°C.

Thus it is evident that the heat radiated from the lamp was insufficient to influence the thermometer, though that from the steam pipe produced a slight increase of temperature. The question then naturally arose: is the snail sensitive to such slight changes? To test this question a snail was selected which had a definitely negative record. It was tried first with light to make sure that it reacted negatively. Then the steam pipe was substituted for the lamp; to this the snail was indifferent. The steam pipe was replaced by the lamp; the snail again became negative. It was therefore evident that the animal is not sensitive to any heat that may radiate from the lamp.

Thus we see that in *Helix* the normal negative phototropism of animals with eyes gives way to a condition of indifference to photic stimuli when the eyes are removed. We may therefore conclude that the eyes of this snail are sensitive to light.

These results agree with those of Willem (1892, p. 83), who has given evidence to show that European specimens of *Helix aspersa* are negatively phototropic. Willem also found that *Helix pomatia*, the species worked upon by Yung, is also influenced by light and tends to gather in brightly illuminated areas. It is therefore quite probable that Yung's contention that the eyes of snails are functionless is erroneous. He based this conclusion partly on the fact that when bright objects were presented to the snails, their tentacles were never retracted until their eyes were actually touched. This merely signifies, however, that the eyes of the snail are of no special value as image-forming eyes. I also obtained similar results from *Helix aspersa*, as well as from frogs, but I do not believe that anyone would doubt for this reason that the eyes of frogs are functional, at least as direction eyes. Yung also tried amputating the eyes of snails and claimed that it had no effect on the behavior of these animals, a result quite contrary to mine.

SUMMARY

1. *Helix aspersa* Müll. is negatively phototropic, at least at certain intensities of light.
2. The photoreceptors are in the eyes at the ends of the dorsal tentacles.
3. The animals are not dermatoptic.
4. The eyes are sensitive only to light falling in a particular direction on the ends of the tentacles.
5. These eyes probably function as direction eyes only.

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INTEGRATION OF MOVEMENTS IN LEARNING IN THE ALBINO RAT

A STUDY OF THE ADJUSTMENT OF AN ORGANISM TO AN ENVIRONMENT

JOHN LINCK ULRICH

VI. RESPONSES TO OBJECTS, OR "DISCRIMINATION"

The importance of the external senses in learning can best be discovered in problems where objects to be responded to are arranged in definite, well exposed positions, such as in problems of "discrimination." In the problems already investigated, less definitely do parts of a problem show that specific sensory excitations produce responses than is the case when two objects must be responded to through visual or other sensory excitations. Usually, in problems of "discrimination," one of two lights of different physical intensities or of different patterns is supposed to be "discriminated" from the other. In such problems, the importance of the mechanism of the senses is implied, or suggested, when stress is laid upon sense functioning and a "habit" of "discrimination" is designated as a "sensory-motor habit" in contradistinction to a motor habit. This supposed distinction between the two "habits" arises because problems requiring a process of manipulation, or of progression, are considered somewhat different from problems of exclusive sense functioning. Such a distinction of "habits," whether made intentionally or not is particularly confusing when all theories of learning are strictly based upon the establishment of "sensory associations," or "sensory-motor connections" in the nervous system.

The reasoning upon which all the theories of learning are based, is that in the early trials in learning a problem, many stimuli are supposed to arouse in the external senses excitations which spread to internal efferent paths which produce many movements. No movement is produced unless through excitation in an external sense organ. Excitations in the external senses produce the "successful" movements, and from that time on with the reduction of the "unsuccessful" movements, sensory excitations which produce the "successful movements" become associated or connected. The idea of "sensory associations" being formed in learning to the exclusion of practically all else, is inseparably bound up with the view of the existence of the "successful" movements. This idea is usually expressed by saying that learning consists of a reduction of the number of the senses used, to the exclusive use of one or a few external senses, and of the "kinaesthetic" senses. This practically amounts to saying that all learning is ultimately the result of sensory "discrimination."

It has been shown that the greatest number of movements are made by rats with undeveloped reflex extensor thrusts, and this accounts for the statement that excitations in the external senses are not always necessary to produce responses. Compensation in the use of a large number of muscles has been observed to be the rule when the functional condition of the organism is not the best. The production of numerous movements without the intervention of sensory excitations may be accounted for by the existence of inadequate inter-connection of efferent neurons in the two half centers in the cord for extension and flexion of the limbs when an unequal antagonistic action of the limbs exists. That efferent interconnections in the cord are of some importance in the production of movements is in accord with the researches of Brown (47) (48) (49) on progression.

It has, however, been observed that in the early trials in learning, certain external senses may be used, and later the behavior of the rat indicates that these senses are not regularly used. The senses are usually reintroduced when a disruption of responses occur in consequence of the appearance of an imperfect response. This temporary use of a number of sense organs does not necessarily increase the number of movements as some theories of learning state, but in addition to the production of movements, increases the extensor tone of many reflexes and increases the efficiency of the rat. This increase in extensor tone of reflexes is most necessary in the early trials of learning. In the early trials in learning the latch-box, the inclined-plane, and the maze problems, touch, vision, and olfaction and internal senses undoubtedly increase the extensor tone of many reflexes, making it possible for excitations in one or more of the external senses to produce adequate responses. Again, if at any time during learning, directive integration is disrupted by the occurrence of imperfect responses, then vision is seen to be reintroduced to facilitate posturing for progression and for the manipulation of the mechanism of a problem. Disruption occurs when inadequate posture with the performance of an imperfect response is produced, before the latch, the plane, or when progression takes place into a cul de sac in the maze. Vision does not direct the rat's course, or directly produce movements, but produces adequate posture, so that movements can be produced in a definite direction.

Many of the foregoing statements were substantiated by additional experimentation with the latch-box problem. These experiments were undertaken to see whether at any time during learning one part of this problem was "discriminated" from another part, or whether the excitations in the external senses increased the reflex tone of an organism in such a way that a definite response could be made. According to this last statement, a movement could be said to be produced when posture had become adequate. A sensory excitation does not directly produce a movement of one part of the rat's body, but adequate posture conditioned by a sensory excitation produces a movement. In previous investigations of learning of the latch-box problem, posturing to produce the neck reflex movement was seen to be more important than excitations received from the latch when it was touched, for repeated touching of the latch did not produce this movement. Posturing was necessary, and it is to be seen if the same thing is not true in so-called problems of "discrimination" when it is supposed that one part of the latch-box problem is "discriminated" from another part.

The experiments with the latch-box problem consisted of two parts: the first part was to introduce the sense of vision by disrupting directive integration after it had been facilitated; the second was to establish, from the first trial, the use of vision for progression to the latch. This disruption of directive integration was easily accomplished, and the use of vision was introduced when the problem was shifted to the right or to the left of its former position. With the first part of the experiment, 10 rats were used that had 60 or more trials in learning the latch-box problem. To bring about the permanent use of vision 8 rats were used. The problem box was before each trial rotated 90 degrees in different directions from the first position. In the first position, the door of the problem box was opposite the entrance box. One trial a day method was used. The rats were 53 days old, when fed for 3 days in the problem in the first position.

Shifting the problem to the right or to the left from a previous position often produced as decided a disruption in directive integration, as when an imperfect response occurred and vision was introduced to reëstablish progression to the latch. When the problem was shifted from 7 to 10 cm. to the right while the experimenter faced the problem, disruptions in directive integration occurred with the introduction of vision to reëstablish progression to the latch. Preliminary experiments with 5 rats determined these distances by shifting the problem from 1 to 2 cm. at a time. Shifting the problem to the right, necessarily produced the greatest disruption, for the latch was on the left side of the door and in direct line with the entrance box. Progression had to be more diverted when the problem box was shifted to the right than to the left. Thus 10 cm. to the left of the old position would accomplish disruption of directive integration, whereas 6 cm. would do the same thing when the problem or latch was displaced to the right. Shorter distances in either direction produced very little effect, only slight disruptions in the attainment of posture under the latch were occasionally noted, and sometimes no change in the movements of the rats could be detected. At any of the maximum distances, disruption of previously facilitated responses occurred. Progression was to the old position of the problem with the production of one or more ineffective neck reflex movements, and then vision or touch was introduced to reëstablish the position under the latch and the attainment of posture to produce the neck reflex to raise the latch; or disruption in directive integration was so great that progression was around the problem and vision or touch reëstablished posture under the latch. This disruption of directive integration continued for several trials when the problem was shifted to 6 cm. to the right and 19 cm. to the left of its old position. Usually on the fifth or sixth trial, directive integration and posturing with the production of the neck reflex movement were once more uninterrupted.

To force the constant use of vision to produce posture under the latch, the problem before each trial was shifted greater distances from the first position. Rotating the problem 90 degrees before each trial was very often found to bring about the use of vision. On one day, the door of the problem box was 90 degrees to the right, on other days 90 degrees to the left, and still on another day 180 degrees from its first position when the door was opposite the entrance box.

In many instances the first trial had facilitated to some extent directive integration to the first position of the door, for on the second trial repeated progression to the previous position of the door occurred, and frequently movements of the head to and fro indicated that vision on the second trial conditioned progression to the door, and the production of posture under the latch. Posturing under the latch in a postion 90 degrees from the first one was accomplished, or took place after some part of the body, principally the vibrissae, touched the door. This behavior brought about movements which culminated in the production of posture and the neck reflex movement. On the third or fourth trial, the behavior of the rat changed; progression around the problem occurred several times and it finally ended, either a short distance in front of the latch or close to the latch. In such cases, movements of the head and contact with the latch resulted in the production of posture and the neck reflex movement. When progression ended near the latch, vision brought about progression with the gradual production of posture. After several trials, the ending of progression before the door became less frequent, and the slowing of progression on nearing the latch was a more regular performance. At the end of thirty trials, vision almost continuously aided the adjustment of the body for progression, and for the production of the neck reflex movement more often than the method of contact with the door by the vibrissae. Frequently, imperfect responses occurred when this last method was used.

With the occurrence of two different methods to attain posture under the latch, it may be said that touch in one instance and vision in another brings about with many other sensory excitations posturing for the production of the neck reflex movement, rather than that one part of the problem is by either method "discriminated" from another part. When progression ends or slows down before the door and vision is introduced, there is a recurrence of the behavior of the rat noted on the second and third trials when learning of the latch-box problem was under investigation; when touch was used to bring about the attainment of posture, there reappeared conditions existing in some rats on these very trials when vision was not used and learning of the problem was difficult. There appears to be no difference in the behavior of rats when the problem box remains in one position, or is rotated before each trial, with the exception that when the problem is rotated, behavior existing in the early trials of learning with the problem box in one position persists.

Apparently, these results show that "discrimination" can be reduced to a more definite explanation than is implied by using the terms "sensory discrimination" or "sensory motor habit." The latch is responded to in the usual way with the problem box in one position, but with the aid of vision to produce direct progression and posture. Sense functioning is not something in a measure apart from the functioning of a more important part, the developed integrated reflex of the rat. It was seen in the investigation of learning in other problems that the receiving of adequate stimulus did not produce a response if the functional condition of integrated reflexes were undeveloped. The same thing is observable in past investigations of "discrimination" when two lighted screens of different light intensities were used. It is evidenced in the great number of trials required, the institution of punishment, and in the average number of "correct" responses made. To substantiate this conclusion, the present extension of investigation of responses to two objects was undertaken. The first objects required to be responded to were a ball and a cube, and the second to two illuminated screens of different light intensities.

The ball and the cube were made of solid maple wood painted a uniform flat light grey colour. The ball was 8 cm. in diameter and the cube 8 cm. square. These two objects were placed in the problem box (fig. 12). This box was different from those usually employed in "discrimination" of patterns or lighted screens, in that progression to the objects was unhampered by any partition separating the objects. These objects were placed in a large open space 115 cm. by 46 cm. which permitted perfect freedom of movement in all directions. The course to the food was not past the objects, as is customary with other problem boxes of a similar kind, but was continued down a hole in front of either object, then through a passage to the food. At the rear end of the problem there were three small compartments connected with each other by sliding doors which could be operated by pulling strings attached to them. Food was placed in either one of the rear end side compartments which were connected by doors with the passage ways leading from the largest compartment through the holes in front of the objects to be responded to. The center rear end compartment, or home box, was connected with this large compartment by a sliding door which likewise could be operated by pulling a string. The entire problem, with the exception of the objects to be responded to, was painted a dull black.

At the time of experimentation a rat was placed in the central rear end compartment, and then it was admitted to the largest compartment by opening a small sliding door operated by a string. The operator stood at the other end of the problem and at some distance from it. Progression to either food compartments could be through either hole under the objects to be responded to and then through a side passage to a food compartment. Admittance to this last compartment was accomplished by pulling a string which raised a door. The movements of a rat were carefully noted and these observations were facilitated by a large mirror placed at an angle of 45 degrees over the largest compartment. The entire problem was uniformly illuminated

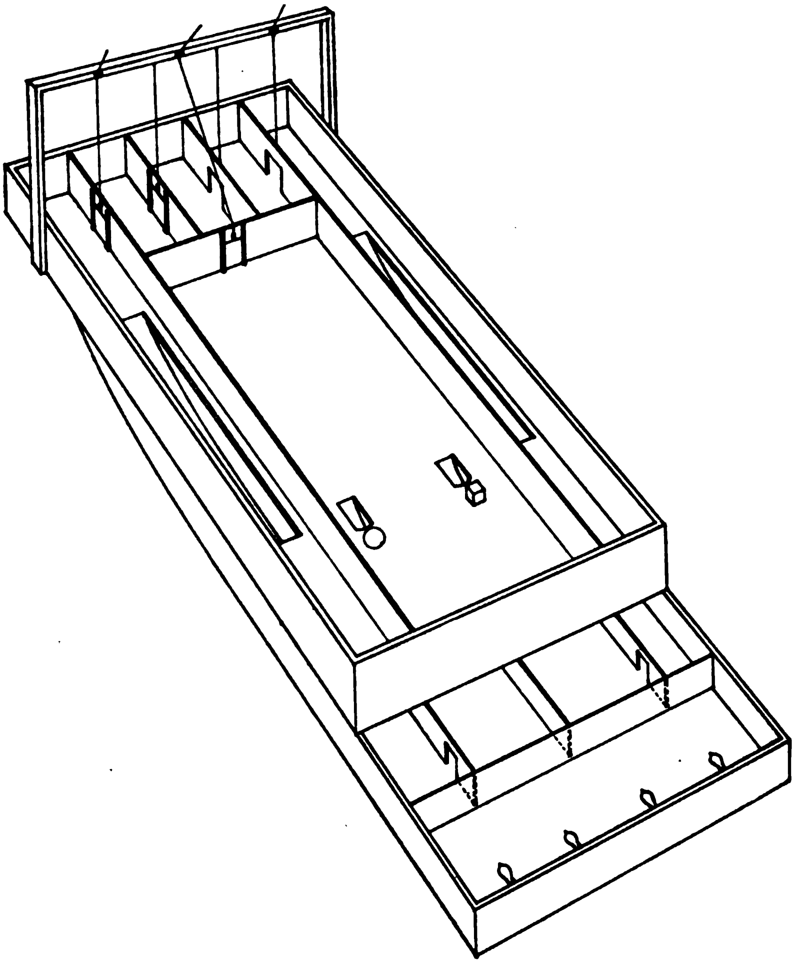

Fig. 12

with electric lights and reflectors, so that no shadows were cast by either the ball or the cube.

Some preliminary work with the ball and cube problem showed that a response to either the ball or to the cube was, in rats, a difficult thing to produce. After a great many trials only an occasional response to either object, or first to one then the other occurred. From almost total negative results, a fair understanding of the problem was obtained. The impossibility of producing a response led to a continuation of investigations, for learning under a difficult situation must be investigated. From an understanding of the limits or the inadequacy of responses produced, the responses to objects can be reduced to a more definite explanation than is implied by the usual statement of a rat's inability to "discriminate" or "establish a sensory motor habit." As previously pointed out, it is from a consideration of negative as well as of positive results that learning can alone be understood.

Since a response produced in a rat is closely linked up with the functional condition of the reflex extensor thrusts, it is once more advisable to correlate their condition with every response made to the ball or to the cube or to both objects. From 150 rats 40 to 60 days old, 11 were selected because of the functional condition of the extensor thrusts of their limbs. In order to determine the degree of fluctuation of the thrusts tests of the thrusts were conducted for 3 days prior to feeding in the problem. Two rats were obtained in which the reflex extensor thrusts promised to be constant. One rat was taken in which no extensor thrusts could be evoked. In the remaining 9 rats a right or a left ipsilateral fore thrust could be evoked, or either of these reflex thrusts could be evoked with a stronger extensor tone than its contralateral. When not very promising results were obtained from any of these rats, another rat with functionally well developed reflex thrusts was selected. With this rat learning the maze problem was completed in 20 trials.

Food was placed in both food compartments during 5 days of preliminary feeding. All the doors leading to parts of the problem box were opened and free access to all parts was possible, so that adjustment to the environment of the problem could be facilitated. The number of days for adjustment to the problem was determined at the time of preliminary experimentation. It was then seen that the most effective responses, including effective progression to the objects to be responded to, appeared often on the second day's or even on the first day's trials, and fewer than 5 days of feeding did not result in a complete adjustment of all rats to the problem box. Adjustment to the problem was thought complete when the first trial showed evidence that direct progression to either food compartment was possible. With some rats, direct progression appeared with fewer days of feeding, but one method of 5 days for all rats will most likely produce comparative results on the first trials.

A number of considerations determined the method employed for investigation of responses to two different objects, the ball and the cube. Much of the work done by other investigators on "discrimination" has been based upon the number of "correct" responses made without the intervention of any known stimulus, other than is supposed to be produced by the objects to be "discriminated." A "correct" response could constitute progression to the food without the production of a definite response to the objects. Preliminary work with the ball and the cube problem, and investigations in learning in other problems, showed that it is the effective performance of definite responses that can be produced in an organism that determines the so-called correct response. It was effective progression followed by the performance of the neck reflex in the latch-box and by the extensor thrust in the inclined-plane problem that determined learning of these problems. In the maze problem, it was effective direct progression through alleys and through turns to a goal that could be said to constitute learning of this problem. Progression, and a definite response to one of two objects when effectively performed must constitute a correct response.

The experimental procedure in the new problem was very simple. Food was associated with the ball. The number of trials given daily varied throughout learning. For the first 10 days, 10 or 20 trials were given, but later 20 to 25 or even 30 were given. The number of trials on some days was increased, but never more than 5. This increase was made when, at the end of a series of trials definite responses to either the ball or the cube or to both tended to appear. Additional trials, it was thought, would develop interaction of body parts for direct progression to the objects, produce effective responses to them, and then progression to the food. The reflex extensor thrusts were tested before each daily series of trials and only occasionally throughout these trials. Tests were not made before each trial, for it was thought that too frequent tests would affect the functional condition of the reflex extensor thrusts, producing more active fluctuations of them. Throughout learning the ball and the cube problem, every means must be considered that will facilitate the appearance of effective responses to the ball, or to the cube, or to both.

When effective responses are recognized, every movement that is produced by a rat is understood. As a result it is easier for the operator to control the movements of a rat. Responses to noises produced as a consequence from shifting the objects to be responded to, or moving the food dish in the food compartments can be determined, and adequate measures to exclude them can be made. And no help such as shifting of cards or "casting of a die" are necessary to establish the position of the ball and cube to prevent the facilitation of a position habit, or the constant progression to one food compartment. Moreover punishment was not used to obtain a maximum of "correct" responses, for some work with punishment showed that it was an extremely artificial way to produce what ought naturally be an effective response. In fact effective responses become less clearly understood, for there are produced exaggerated effective responses to the objects. Every "natural" movement produced in the environment of the problem is of more interest and significance in producing a cumulative effect on the organism or increasing the mechanical efficiency of the organism than those produced by excessive stimulation.

During the first 10 trials, direct progression to the objects to be responded to, or directive integration, is facilitated. Progression is not direct during the first few trials, but short and long spurts are made. Sometimes retracing of steps to the door of the home compartment occurs. Usually the same path is taken for several trials to one food compartment even though food may not be present in it. Occasionally the following of one path becomes so persistent that it is advisable to cover the hole which leads to the compartment. When progression through the other hole occurs, the cover on the first hole can be removed. Usually then progression, alternating through one hole then another, is facilitated. Direct progression through either hole for a trial or two, without obtaining food, produces a change of behavior in a rat. Ineffective progression and responses to the objects appear, and objects are touched, smelt, and gazed at. Progression to a food compartment may then be diverted in the direction of either object. With most rats, such behavior occurs during the first 10 daily trials. Sometimes when 20 trials are given, effective responses either to the ball or to the cube may be produced.

In all problems investigated, ineffective movements of one kind or another precede the actual performance of the important movements that solve the problem, and in many cases precede the occurrence of imperfect responses. The same thing occurs in a problem requiring responses to two different objects such as the ball and the cube, but with this problem it can not be said that imperfect responses are produced. Instead, constant progression to one food compartment is likely to be established, or wandering progression in place of direct progression occurs. Ineffective responses to the ball or to the cube or to both objects appear to be more in the nature of imperfect responses, but these responses seem to be but an early facilitation of the attainment of posture for a response to the objects. The apparent absence of imperfect responses is possibly due to the existence of a greater freedom of movement in this last problem than is permissible in the other problems investigated.

Responses to the ball, or to the cube, or to both are produced more numerously by rats as the reflex extensor thrusts appear to be better developed. Only in one rat were effective responses to the objects produced for many trials, and this rat had previously learned the circular maze in 20 trials. In this rat the reflex thrusts were well developed, not fluctuating to any perceptible extent and reflex excitability was manifest. The other rats, selected with apparently fully developed reflex thrusts, showed, after a few trials, that the thrusts were not constant but fluctuated, and often only one ipsilateral fore reflex thrust could be evoked when the rat's head was bent to the right or to the left of the midline of the body. With some of these rats, before fluctuations became active, responses to the objects occurred from time to time. With other rats, in which at the outset the reflex extensor thrusts were functionally undeveloped, very few responses appeared, and these were usually ineffective. Thus the number and the effectiveness of the responses to the two objects, the ball and the cube, are dependent upon the development of the rat's integrated reflexes, and not exclusively or primarily upon the external senses or specific sensory motor connections. The mechanical efficiency of the organism in an effective adjustment to an environment is an important factor.

When the reflex extensor thrusts are fully developed, as was the case in the rat which had previously learned the circular maze problem, responses to the objects appeared in the first 20 trials. On the second series of 20 trials, the responses to the ball or to the cube or to both objects appeared still more definite. Progression to the objects and to the food was not rapid, but slow to the ball or to the cube, or first to one and then to the other, and then more rapidly to either hole to the food. The external sense, vision, at all times determined progression to the objects, and either one or both of them were touched by the vibrissae, or by the putting of both fore feet on them.

At times, olfaction was noted to produce a change in the direction of progression through either hole to either food compartment. Sometimes it appeared difficult to distinguish whether the senses olfaction or touch by the vibrissae or both were functioning, however, when progression was close to the objects and the vibrissae came in contact with them, it was safe to conclude that the sense of touch by means of the vibrissae functioned and not olfaction. During this second series of 20 trials vision determined progression to the ball; it was touched on two different trials by the feet and adequate responses to the ball followed. Once vision produced an inadequate response. On the remaining trials, direct progression to the food occurred, or inadequate or ineffective responses to the objects were produced.

During succeeding series of trials on other days, an increase in the number of responses appeared, and those produced by touch of the forefeet predominated. These responses become more frequent when the ball is alone touched, or when it is the last of the two objects touched, and they are less often produced when the cube is alone touched, or after the ball is touched. Vision is only at times effective in producing responses. Up to the five hundred and sixty-fifth trial, many responses to the objects appear. The adequate responses to the objects that leads to the food comprises about half of the number given in one day's series of trials. These adequate responses are produced by vision or by touch of either the ball or the cube, but more often the stimuli from the ball is effective. At the end of the 1400 trials, vision determined the greater number of adequate responses. Table 36 gives some idea of the gradual development of adequate responses to the two objects. Direct progression to the hole underneath the ball without a response to the ball is recorded with inadequate responses. Such direct progression could not alone be detected by the speed of the rat, but also by the position of the rat's head which was lowered some distance before the hole was reached. There was no apparent difficulty in distinguishing rapid direct progression to the food, and slower progression to the ball and a response to it, before continuation of progression to the food.

Experimentation was continued to 1740 trials, but a great number of adequate responses were not produced until 1390 trials. From 1390 to 1415 trials, 21 adequate responses appeared. The records on some days were often inferior to other days. This variation in the records appeared almost invariably every other day, and seemed to indicate an absence of efficiency in the organism after effective production of responses on a preceding day. From the movements of the rats, it was quite apparent that the effectiveness of reflexes for posturing for progression and for producing adequate responses was lacking. It was at times evident that vision or touch could not increase the extensor tone of certain muscles to produce adequate responses.

It appeared in the early trials, vision must be supplemented by touch of the feet or of the vibrissae. The gradual increase in the use of vision was shown in tables 37, 38, and 39, which are the records of three different days. Numbers 1 and 2 on these tables refer to the positions of the ball and the cube in the problem box. When the ball was in position 1, it was to the left of the operator and in position 2 to the right of the operator. When a statement accompanies a record, it indicates that an adequate or an inadequate response was made to either the ball, or the cube, or to both objects.

TABLE 36

TRIALS	TOUCH FEET	TOUCH VIBRISSAE	VISION	ADEQUATE RESPONSES	INADEQUATE RESPONSES
1–20*	Both 4	Both 1	Both 4	3	17
20–40	Ball 2, both 2		Ball 4	5	15
124–144	Cube 2, ball 4, both 1		Ball 2	7	13
320–345	Ball 12, cube 4			12	13
410–435	Ball 7, both 11, cube 1	Ball 1, both 5, cube 3	Ball 1, both 2	14	11
545–565	Ball 5, both 4	Ball 1, both 3, cube 2	Both 1	11	14
630–655	Ball 2	Ball 6, both 2, cube 2	Ball 2, both 1, cube 3, vision and touch 4	19	6
847–872	Ball 8	Ball 1, cube 1	Both 7	15	10
1216–1241	Ball 1, cube 1	Ball 3, cube 5	Ball 8, cube 2	16	99
1390–1415	Ball 2, both 2	Ball 1	Ball 11, cube 6	20	5

The first of these tables, table 37, shows that from 410 to 435 trials responses either to the ball or to the cube or to both occurred at different times. Responses were produced more often to both objects, than to either one of them, and when touch produced adequate responses, the ball was the last object touched.

When vision was used, the head was moved in the direction of the ball then to the cube, and an adequate response was usually produced. The most immediate and effective responses were those produced following touching of the ball by the feet. On the second of these tables, table 38, which gives a day's series of trials from 630 to 655 trials, rather different results are presented. In this table, it is recorded that vision more often produces adequate responses, and chiefly from stimuli received from the ball and the cube. Touch of the ball or the cube by the vibrissae produces few definite responses. Both objects are responded to in one instance when vision and in another when touch is used. Either object alone can now produce an adequate

TABLE 37

TRIALS	
410 − 2	
411 − 1	Response to touch of the feet to ball then cube
412 − 2	Response to touch of the vibrissae to cube
413 + 2	Response to touch of the vibrissae to ball
414 + 2	Response to touch of the feet to ball then cube
415 − 2	Response to touch of the feet to ball then cube
416 − 1	Response to touch of the feet to ball
417 − 2	Response visual to ball then cube
418 + 1	Response to touch of the feet to cube then ball
419 + 1	Response to touch of the feet to ball, cube then ball
420 − 2	Response to touch of the feet to ball, cube then ball
421 + 1	Response to touch of the feet to ball
422 − 2	Response visual to ball and cube
423 − 2	Response to touch of the vibrissae to cube
424 + 1	Response to touch of the feet to the ball
425 + 1	Response to touch of the feet to the ball
426 + 2	Response visual to ball then cube
427 − 2	Response to touch of the feet to cube
428 + 1	Response to touch of the feet twice to ball
429 + 1	Response to touch of the feet to ball
430 + 2	Response visual to ball and cube
431 + 2	Response to touch of the feet to cube then ball
432 − 1	Response to touch of the vibrissae to ball
433 + 1	Response visual to the ball
434 − 2	Response to touch of the vibrissae to the cube
435 + 2	Response visual to the ball and cube

response for the continuation of progression to the food. Adequate responses to one object are most evident on the next table, table 39, which gives a series of trials from 1390 to 1415 trials. In the beginning of this series of trials, touch by the feet or by the vibrissae produced responses, but vision is recorded to have produced the greater number of responses and chiefly from stimuli received from the ball.

TABLE 38

TRIALS	
630 − 1	Response to touch of the vibrissae to ball
631 − 1	Response to touch of the vibrissae to ball
632 + 1	Response to touch of the vibrissae to cube
633 + 1	Response to touch of the vibrissae to ball
634 + 2	Response to touch of the vibrissae to cube
635 − 2	Response to touch of the vibrissae to cube
636 + 1	Response to touch of the vibrissae to ball and cube
637 + 2	
638 + 2	Response to touch of the vibrissae to ball
639 + 1	Response visual to ball and touch of the feet to cube
640 − 1	
641 + 2	Response visual to cube
642 + 1	Response to touch of the vibrissae to ball
643 + 1	Response to touch of the vibrissae to cube and ball
644 + 2	Response to touch of the vibrissae to ball
645 + 2	Response visual to cube
646 − 1	
647 − 2	Response visual to ball and to touch of the cube
648 + 1	Response visual to cube and to touch of the ball
649 + 2	Response visual to cube
650 + 2	Response visual to ball
651 + 1	Response visual to ball
652 − 1	Response to touch of the feet to the ball
653 + 2	Response to touch of the feet to the ball
654 + 1	Respinse visual to cube and to touch of the feet to ball
655 + 1	Response visual to ball and to cube

From the first trials in the ball and cube problem, touch frequently produced responses and it is again employed in the first of each day's series of trials. Later vision is resorted to in the remaining trials. Touch seems to be less adequate to produce responses than vision, and this conclusion is reached not only because of the rapidity with which the responses are made with the use of vision, but because many more consecutive adequate records are made. Again with the use of vision, responses at the end of learning are made to one of the objects. Because the ball is in direct line with the food, more rapid responses are made to it than to the cube. There appears to be as a result no so-called "discrimination" of the two objects, but a tendency to produce responses in as definite a manner to one object as is made to the latch of the latch-box, or to the plane of the inclined-plane problem. There appears to be no difference in the way a response is produced to the latch, the plane, or the ball. Progression to all these objects is to a definite place, posture is produced, and another response is made which results in a contin-

TABLE 39

TRIALS	
1390 − 2	
1391 − 1	
1392 + 1	Response to touch of the feet to ball
1393 − 1	Response visual to ball
1394 + 2	Response to touch of the feet to ball
1395 + 2	Response visual to the ball then to touch of the feet to cube
1396 − 1	Response to touch of the vibrissae to ball
1397 + 1	Response visual to ball
1398 + 1	Response visual to ball and the cube
1399 + 2	Response visual to ball
1400 − 2	Response visual to ball
1401 + 1	Response visual to ball
1402 + 1	Response visual to cube
1403 + 2	Response visual to cube
1404 + 2	Response visual to ball
1405 + 1	Response visual to ball
1406 + 1	Response visual to ball
1407 + 2	Response visual to cube
1408 + 2	Response visual to ball
1409 + 2	Response visual to ball
1410 + 1	Response visual to cube
1412 + 2	Response visual to ball
1413 + 1	Response visual to ball
1414 + 2	Response visual to cube
1415 + 2	Response visual to ball

uation of progression to the food. In fact posture for progression to any of these objects passes into posture to produce a response to these objects, and once more posture is attained for progression to the food. In the ball and the cube problem, as in the latch-box problem when rotated, vision is forcibly introduced.

There is much difference in the results obtained from rats in which the reflex extensor thrusts of the rat's limbs are undeveloped. Two rats selected with functionally developed thrusts and in which in a few trials these thrusts fluctuated, produced rather indifferent results. Fluctuations of these thrusts in one rat was so marked that very few responses were produced to the ball, to the cube, or to both objects; but from the other rat rather interesting results were obtained, which supported the conclusion arrived at from the results obtained from the rat of the preceding experiment.

One of the rats, in which there could be evoked in the beginning of learning the problem, well developed reflex thrusts of the limbs, in later trials when the rat's head was passively bent to the right or to the left, the extensor tone of the right ipsilateral fore thrust was stronger than the left and the right fore thrust was alone evoked, or simultaneously with the evoking of the right fore thrust its contralateral was also evoked, and when the head was extended dorsally 45 degrees of the midline of the body either a left fore ipsilateral thrust or none was evoked.

This rat was given 1740 trials. During the first 260 trials all reflex thrusts were evoked, and at the three hundredth trial the thrusts began to fluctuate slightly. The results obtained prior to active fluctuations of the extensor thrusts were promising for the first 20 trials, and several of other series of 20 trials given daily, showed as many responses to the objects as that of the first rat. Either touch or vision produced these responses. With 400 trials these responses were increasing and on the last of these trials, it was noted that the reflex tone of the right ipsilateral fore thrust was weak and the left fore thrust could not be evoked. The extensor tone of the hind limbs was also weak. Vision to produce responses was rarely employed. This may be seen on table 40. Very few consecutive responses to the ball or to the cube or to both objects were produced.

In later trials, adequate visual responses to the objects do not increase in number. The same thing can be said of touch, but olfaction produces in an inadequate way responses to the objects. Table 41 gives the record from 1640 to 1665 trials. This record is an exceptionally good one; those of other days frequently showed but an occasional response to either object. On such days progression is more often direct to either food compartment, and no evidence of a response to the objects can be detected. This behavior is particularly true when a test of the reflex thrusts shows marked fluctuations before a day's series of trials begins. On the other hand when the extensor tone of the limbs are stronger than on other days, the behavior of the rat is quite different.

TABLE 40

TRIALS	
370 – 2	
371 – 1	
372 – 2	
373 + 2	Response of the touch of the vibrissae to cube then to ball
374 – 2	Response visual to cube and to ball
375 – 1	Response of the touch of the vibrissae to cube
376 – 2	Response of the touch of the feet to cube
377 – 1	
378 – 2	
379 – 1	Response of the touch of the vibrissae to cube then to ball
380 – 2	Response visual to cube
381 – 1	Response of the touch of the feet to cube
382 – 2	Response visual to cube
383 + 1	Response of the touch of the feet to cube
384 + 1	
385 – 2	
386 + 2	Response of the touch of the feet to ball
387 – 2	
388 – 1	Response visual to cube
389 + 2	Response to touch of the feet to ball
390 + 1	Response to touch of the feet to cube
391 – 1	Response to touch of the vibrissae to ball
392 – 2	Response visual to cube and touch of the vibrissae to ball
393 + 1	Response visual to ball
394 + 1	Response visual to ball
395 – 2	

Frequently, then, progression slows down as the objects are neared, the head is moved in the direction of either the ball or the cube, and instead of visual excitations producing a response which would continue progression to a food compartment, as occurred with the preceding rat, progression is to the ball, or to the cube, or to both objects, and olfaction produces an adequate or an inadequate response. At other times, progression is to one or both objects and olfaction produces a response. Only on the first two trials are the objects unresponded to. Of the 25 trials given on this day, 19 adequate responses were produced, most of them by olfaction, vision producing only 2.

TABLE 41

TRIALS	
1640 − 1	
1641 − 1	
1642 + 2	Response olfactory to ball and to cube
1643 + 2	Response olfactory to cube
1644 + 1	Response visual to cube and olfactory to ball
1645 + 1	Response visual to cube and olfactory to ball
1646 + 2	Response visual to cube and ball
1647 + 1	Response visual to cube and olfactory to ball
1648 + 2	Response olfactory to cube and to ball
1649 + 2	Response olfactory to ball
1650 + 1	Response olfactory to ball
1651 + 1	Response visual to cube and olfactory to ball
1652 + 2	Response visual to cube and olfactory to ball
1653 + 2	Response visual to cube and to ball
1654 + 1	Response olfactory to ball
1655 + 2	Response olfactory to ball
1656 + 2	Response olfactory to ball
1657 − 2	Response olfactory to ball and to cube
1658 + 1	Response olfactory to ball
1659 + 2	Response olfactory to ball
1660 + 1	Response olfactory to ball
1661 + 1	Response olfactory to ball
1662 + 1	Response visual to cube and olfactory to ball
1663 + 2	Response olfactory to cube and to ball
1664 + 2	Response olfactory to cube and to ball
1665 − 2	

When comparing the results of these two rats, it can be seen that when the reflex extensor thrusts of the rats are functionally developed, vision can produce adequate responses to the two objects, the ball or the cube, whereas when these thrusts are undeveloped, olfaction produces these responses to the objects. In the second rat as long as the reflex tone of the rat's limbs remained strong, the thrusts did not fluctuate, then touch with the forefeet produced responses, but as soon as active fluctuation appeared, touch was practically excluded and olfaction was employed. Apparently according to the development of the extensor thrusts, *different* external senses are used; in fact, the use of a *particular* external sense is determined by the development of these thrusts. Accordingly it is impossible to conceive that the external senses are particularly involved in "discrimination" of the ball and the cube, or act in any sense in a differential and in an exclusive way in learning.

Nor is one sense organ employed in a more satisfactory way than another in producing responses to the objects when these thrusts are undeveloped. If the table 41 is once more consulted, it will be noted that when vision can not produce a definite adequate response to the ball, to the cube, or to both, then vision is often supplemented by olfaction. Again if the responses produced by the last sense are carefully observed, it will be noted that posturing is quite absent and the responses made to the objects are not produced as rapidly as those produced by vision or even touch. The difference in the responses results not from a difference in the effectiveness of the different senses employed, but in the degree of the development of the reflex extensor thrusts and their interaction with other body parts. A more specialized sense, vision, can produce adequate responses when the functional development of the organism is the best. The limitation of the functioning of any sense is dependent upon the development of an organism.

In the first rat adequate responses could be made to one object alone, to the ball or to the cube, more often to the ball. At times both objects were responded to either the ball first and then the cube or vice versa. In the second rat, many more responses by olfaction were made to one of the objects than to both and more often to the ball. Both objects were responded to when vision failed, and olfaction was supplemented to produce a response. When one object is inadequately responded to by touch or by vision, a response to one must be supplemented by a response to the other before a definite adequate response can be made. This seems to be the case when either the ball or the cube is the last object responded to before progression is continued to a food compartment. A definite response to the cube, when it is the last object responded to, does not imply, it seems, a facilitation of differential responses to the two objects, but since either object alone can produce an adequate response an inadequate one to the ball can result in the cube producing an adequate one. A qualitative sensory difference produced by the objects, resulting in different responses to the two objects is too much to assume. Rather any difference in responses made comes from the degree of development of the reflex extensor thrusts.

TABLE 42

NUMBER	REFLEX THRUSTS			
	Fore	45 degrees	Hind	
1	Both wk.	None	Strong	13 adequate responses at 700 trials and 5 at 1000
2	Both wk.	None	Strong	6 adequate responses at 300 trials and 0 at 560
3	Right wk.	None	Strong	Only inadequate responses at 850 trials
4	Right wk.	None	Strong	No responses at 120 trials
5	Left wk.	None	Strong	No responses at 120 trials
6	None	None	Strong	No responses at 126 trials
7	None	None	Strong	No responses at 120 trials
8	Left wk.	None	Strong	No responses at 100 trials

The results obtained from the 8 additional rats presented in table 42 substantiates this last statement. The reflex thrusts in these rats were poorly developed, and the degree of development determined the production of adequate responses to the ball, to the cube, or both objects. Occasionally such adequate responses were made to these objects. The rapidity and the directiveness of progression to the food was lacking after these occasional responses. The first rat's record summed up on this table, shows that 13 fairly adequate responses were made up to 700 trials and from 700 to 1000 trials only 5 additional responses were produced. This record is the best on this table. The second rat's record reveals only 6 adequate responses up to 300 trials and none thereafter to 560 trials. No adequate responses are recorded for the third rat, but occasional inadequate responses to the objects appeared. Of the other 5 rats, not even inadequate responses appeared and experimentation with them was discontinued after 100 or 126 trials were given. With these last 5 rats no excitations in any external sense could produce even an inadequate response to the two objects.

Before the external senses can produce responses, the tone of integrated reflexes to produce posture must be increased. Posturing is invariably more readily attained in rats manifesting reflex excitability, but it is impossible of attainment to produce responses to the two objects when the extensor thrusts are undeveloped. A few of the rats manifested hyponormal excitability; progression was generally slow and undirected. When excitability was hypernormal and the reflex thrusts undeveloped, progression was most often rapid to either food compartment and no responses to the ball or the cube were made. Posturing for progression and for the production of a response must take place before excitations in any sense organ can change the direction of progression or produce a definite response to an object. With posture attained, a response is produced by the tone of the integrated reflexes involved in its production being increased by a sensory excitation. The *whole organism* is thus involved in the production of a definite response. How far this statement may be supported by the facilitation of responses to two lighted screens of different light intensities is now to be seen.

The problem box (fig. 13) was used to produce responses to lighted screens of different light intensities. This box was a modification of the one used for the ball and the cube problem. The food compartments were enlarged. Light screens 7.5 cm. in diameter were placed in front of two dull black brass tubes

30 cm. long extending into the problem box about 20 cm. above and 18 cm. in back of the two holes which led to the food compartments. The screens consisted of round pieces of "cornig" daylight glass inserted in one end of the tube and held in place by two brass rings. Over the other end of the tubes was fitted a close detachable shield to which were attached electric sockets.

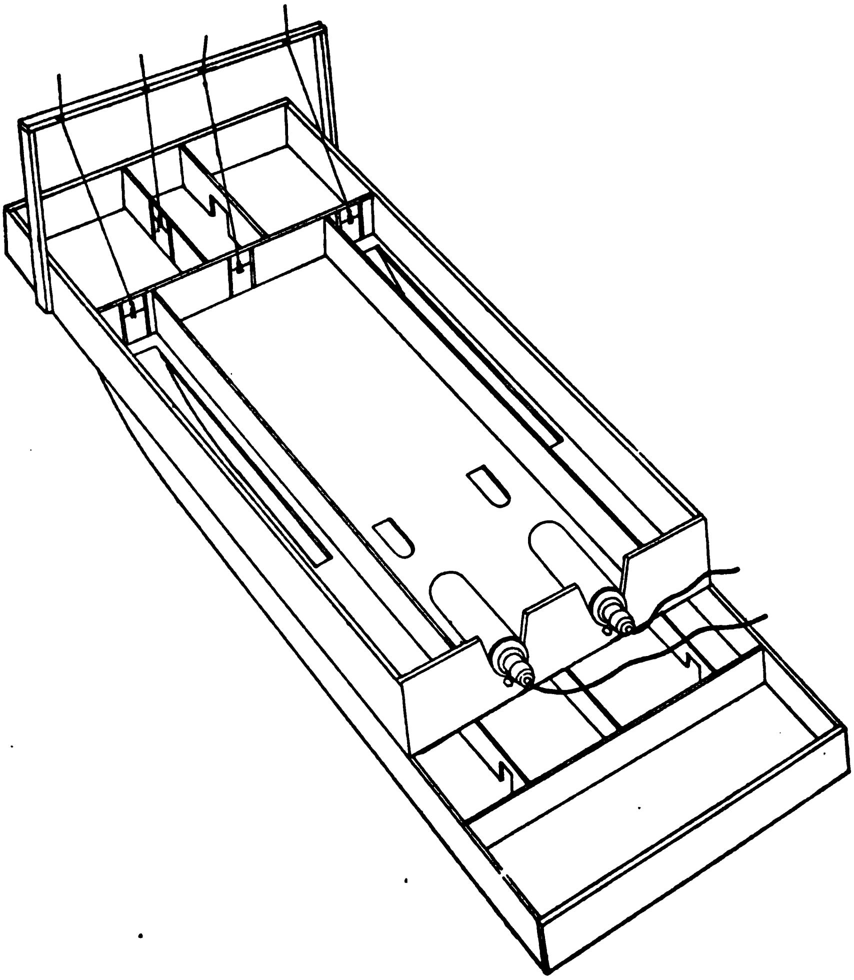

FIG. 13

Into one of the sockets was placed a 4 cp. lamp and into the other 8 cp. lamp. The 4 cp. lamp was connected in circuit with a rheostat to reduce the intensity of the light coming from this lamp. No rays of light escaped from the tubes except through the glass discs or screens. The light issuing through the screens illuminated very little of the problem box, and thus it was not intense. The two brass tubes fitted into two large slots cut into one end of the problem box and rested on a soft cushion of leather. The position of the tubes could be changed between trials without the production of any noticeable vibrations or noise. No partition separated the two tubes or lighted screens, and perfect freedom of movement was permissible in front of and to the side of the tubes.

The physical intensity of the light was not measured, for the object of this experiment was to investigate the responses made to two screens of different light intensities, and not to correlate the physical intensity of the light with any so-called sensory discrimination. In view of the results obtained from the ball and the cube problem, accurate measurements of light intensities would not have any practical value, since responses to objects are not the result of differential changes in the sense organs, but are dependent upon the functional development of the extensor thrusts of an organism. It was recognized, however, that a certain difference in the intensity of the two lights produces many more adequate responses to one of them than when too little difference exists. Preliminary work with 6 rats indicated that responses to the light of greater intensity with which the food was connected, would most likely take place when the difference between the two lights was such that these responses appeared on the first day's series of 20 trials. When this difference in the light intensities of the screens was supposed to be adequate, intentional slight changes were made in the intensities of the two screens but this produced no noticeable effect on the production of adequate responses; only when marked changes in the differences in screens were made, was an effect produced on the rat's responses to them or to one of them. Slight changes in the intensity differences of the lighted screens had no perceptible effect, whereas a slight fluctuation in the reflex thrusts often produced a profound effect on the rat's responses.

When the best intensity difference of the two lights was obtained, experimentation began with 5 rats. These rats were 40 days old when fed for several days in the problem box. These 5 rats were selected from 200 or more for the best functional development of the reflex extensor thrusts. Only one rat was discovered with the reflex thrusts fairly well developed. The other rats possessed an asymmetrical development of them. The reflex tone of existing extensor thrusts remained with the exception of one rat fairly constant, that is, fluctuations in the thrusts were not marked.

During experimentation food was connected with the screen of greater light intensity. For the first 6 days, 10 trials daily were given, and thereafter from 15 to 20, the number of trials depending upon the appearance of adequate responses at the end of a day's series of trials. If adequate responses were made to one lighted screen at the end of 15 trials 5 additional trials were given for that day. The same procedure was adopted with the ball and the cube problem in order to facilitate the development of interaction of body-parts for the production of responses. Fewer trials a day were given in this problem than in the one of the ball and cube. This was done because one of my students has shown with the maze problem that an increase in the number of trials at one period increases fluctuations in the reflex extensor thrusts and consequently many more imperfect responses were produced. All rats were given a total of 700 trials. This number was thought sufficient to determine the conditions necessary for the production of responses to either of the lighted screens.

The reflex thrusts in all rats remained constant for the first 50 or 60 trials, when slight fluctuation appeared. In one rat the reflex tone of all reflex extensor thrusts became weaker but fairly symmetrical conditions prevailed in the limbs on both sides of the rat. During the first 60 trials, responses to either of the lighted screens or to both appeared. From 50 to 700 trials many more of such responses occurred in the rat with the reflex extensor thrusts fairly well developed. In one rat the reflex extensor thrusts had completely disappeared, and an apparent symmetry of both sides of the rat's body was thereby established. Their disappearance resulted for a time in an increase in the number of adequate responses. The number of adequate responses produced in the other 3 rats were very few. Tables giving the number of these responses will not be given, for this part of the experiment was to determine the nature of the responses to objects and what conditioned them.

When two lighted screens of different light intensities are presented to be responded to, vision alone produces the responses to either lighted screen or to both which continues progression to the food compartments. These responses are no different from those which vision alone produced to the ball or to the cube or to both objects. Either lighted screen or both screens were responded to in the same manner as to the ball and the cube. Again, more adequate responses were produced to the lighted screen of greater intensity connected with the food. The adequacy of the responses to the screens again could be said to result in a continuation of progression to the food and not because of any sensory differentiation of the two objects. It was observed that both inadequate progression to the lighted screens as well as inadequate responses to one or to both of them produced imperfect records. Effective posture for progression, and once more effective posture for the responses to the objects were essential for the production of perfect records. At times it appeared an ineffective functioning of other parts of the rat's body as well as the reflex extensor thrusts conditioned the absence of production of responses to the lighted screens, but these parts could not be determined. Usually when posturing was inadequate, rapid progression to either food compartments without any response to the screens appeared. At times when the door of the home box was raised, it was noted that inadequate posture for a start for progression existed.

Posturing for progression to the lighted screens is observable in the slight crouching of the rat's body and the raising of the head. Usually, as the objects are neared, progression slows down, and the body becomes slightly more crouched. The head then moves from one object to the other or remains in direct line with one object. Responses are made in the first instance to the last object to which the head is bent or to the screen toward which the head was steadily fixed, and a response is made which produces the continuation of progression to a food compartment. Adequate responses are made when either one or the last of the two lighted screens is responded to and progression is to the food.

Again in experimentation with the lighted screens of different intensities, there does not appear to be exclusive sense functioning in responses to them. A difference in response does not result exclusively from differential excitations quantitative or qualitative in the rat's eye, but is dependent upon the functional development of integrated reflex mechanisms interacting with other reflex parts for the production of posture for progression and for adequate responses to one of two objects. Excitations from manifold senses, vision, olfaction, and internal senses produce posture for responses to objects; but all these excitations are effective in accordance with the functional development of integrated reflexes. This view is supported from the observations and the results obtained from the rotation of the latch-box, from the ball and the cube and from the lighted screens of different light intensities. The "discriminative" responses are not conditioned by sensory association, or by the creation of sensory motor tracts, or by reintegrations in the nervous system, but upon the functioning of the entire organism, some parts of it are more effectively used than other parts. Whatever is implied by the words "sensory motor habit" or "sensory discrimination," apparently qualitatively different responses do not appear, but a more effective response to one object, and, if this is impossible, to another object or to more than one object. Direct progression, when it can occur to the right or to the left at a turn to the goal of the maze, may take place in one direction or in another determined by the functional condition of the entire organism. Peripheral stimuli from the turns or from the objects produce excitations in sense organs which acting upon the centers for progression produce extension and flexion of the limbs and acting upon other parts of the nervous system produce changes in the organism.

These sensory excitations are at all times subordinate to the existing functional condition of the integrated reflex mechanisms. Sensory excitations produced by stimuli from one, or from one of two objects result in an exaggerated performance of the fundamental activity of extension and flexion of the limbs in rhythmic progression, in the fundamental movements of the head, and in the production of posture. These movements are inherently coördinated, for they are characteristic responses made to strange objects by some rats when such objects are thrown in the rat's living cage. In a problem box exaggerated performance of movements must take place from trial to trial. Visual discrimination is not in any sense an exclusive physiological process in which the eyes and the nervous system are especially concerned.

VII. DELAYED REACTION

Another problem which has been supposed to be closely connected with sense functioning in learning is that of delayed reaction. An animal which has learned to respond "correctly" to two stimuli, light and food, is after the removal of the light stimulus and a brief period of forcible delay, free to respond to the food alone. It is not the length of the period of delay that is significant, but a difference in behavior with the occurrence of a significant number of "correct" responses, is a justifiable conclusion that qualitatively different sensory processes occur in animals which condition "correct" responses. The maximum delay for a dog may be greater than that for some raccoons and young children, yet the behavior of the dog is quite different from that of the other two organisms. Because of a greater freedom of movement with raccoons and children, during the period of forcible delay, and on the other hand, the attainment of a more or less fixed position or constant direction of movement in rats and dogs during a period of delay, there is warranted a different explanation for the two groups of animals when a "correct" response is made. With raccoons and children, where a greater freedom of movement is observed, the explanation given for the occurrence of a "correct" response to the food, is that at the time of release from delay, there ensues not a reëstablishment of previous sensory functions or excitations of light, but there arises a "content," a representative factor which is supposed to be "kinaesthetic" and is associated with light. These "kinaesthetic" sensations or excitations differentiate responses, guide them, and the "correct" response is produced. They function as an idea. On the other hand with rats, cats, and dogs where greater freedom of movement is not observed to be present, a "motor adjustment," persisting during the period of delay, precedes such a response to the food. In one instance a "kinaesthetic" factor is assumed to direct progression to the food, and in the other, previous "motor adjustment." The most complete investigation of this kind has been done by Hunter (50).

The distinction made in this explanation is rather arbitrary, for if "kinaesthetic" senses, proprioceptors, function when they fulfill an "ideational function" they are certainly known to be active in "orientation" or the attainment of posture. Possibly from the greater freedom of movements, and from generally a greater delay in raccoons and children, these senses may be conceived in any analysis to function with greater freedom in these organisms than where a constant direction of movement of posture in rats, cats, and dogs is supposed to be the rule. This would mean that there exists a greater specialization, or different degrees of development of the proprioceptors in different animals based upon a difference in the behavior of them, and such a supposition is supposed to justify the existence of a non-mechanistic factor which initiates or controls direct progression to the position of the absent stimulus and to the food. This explanation is based upon the idea of the almost exclusive functioning of the senses apart from the rest of the organism, and consequently the complete organization of the organism is disregarded. No difference exists in this explanation for the activity of an organism, from those theories of learning based on "sensory association." Instead of a pleasurable protreptic tendency being present which directs the animal to the food, there appears to be emphasized a greater "sensory intensity" or a "qualitative sensory excitation" generated in proprioceptors. All this analysis arises because of greater freedom of movement manifest in some and not in other animals, when the light associated with food is turned off and direct progression to the food is for a time prevented.

From the results obtained from the previous investigations of responses to two objects, it was observed that the responses made to an object were far more significant than the use of the external senses to produce the responses. It is also conceivable that likewise in a problem of delayed reaction, a difference in movements made by animals in the release box is dependent upon the functional development of integrated reflexes. The problem then becomes one of the performance of adequate responses not only in different animals, but also in the same species of animals as the rat. Though rats alone were used in this investigation of delayed reaction, the results obtained can be applied to quadruped animals and possibly to human beings, for rhythmic extension and flexion of the limbs exist in all these problems. If the development of the mechanism for progression can account for manifestations observed at the time of forcible delay in the release box, then any suggestion of a representative factor may with advantage be disregarded.

The delayed reaction problem (fig. 14) was larger and slightly different from the one used by Hunter and other investigators of this problem. Three boxes each containing two compartments instead of one were placed in the usual manner in three different directions from the release box. One compartment contained a 50-watt lamp, and was connected by a door with the main part of the problem and also by means of another door with a small adjacent food compartment. Direct progression from the release box to the food was to one lighted compartment and through it to the food box.

The rats selected for investigation of delayed reaction were tested for 3 days for the functional development of the reflex extensor thrusts. Several rats were taken with fully developed extensor thrusts, and some with a right or a left ipsilateral fore thrust stronger than its contralateral. The reflex tone of the hind reflex thrust was generally strong. A total of 12 rats were selected. They were 40 days old when fed for 3 days in the problem. Some of the rats manifested hyponormal reflex excitability.

The procedure adopted to facilitate delayed reaction was very simple and varied little from that used with other problems. Feeding in the problem box was continued for 3 days so the environment of the problem could be learned. The light was turned on in all 3 sets of compartments. The usual behavior of rats during preliminary feeding was manifest as in preceding problems investigated. After the environment of the problem was

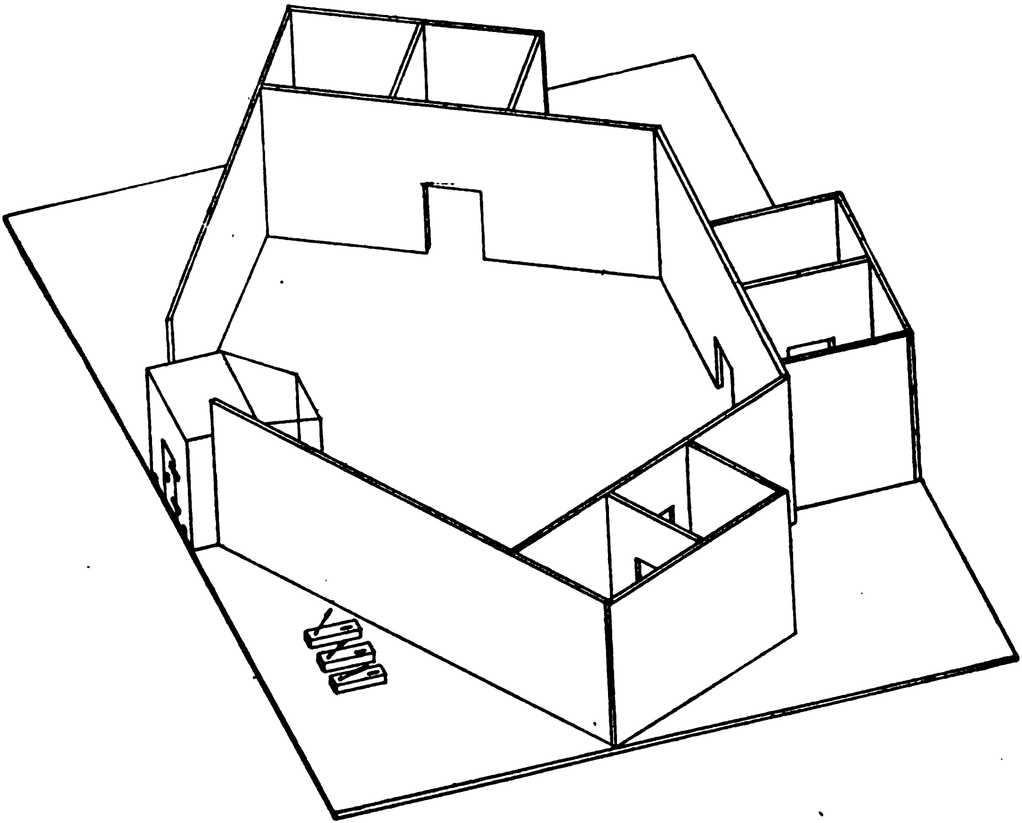

Fig. 14

learned, the facilitation of direct progression first to one then another of the compartments was started. Light was present in only one set of compartments at a time. When progression was direct to all sets of compartments, a different method was instituted; the light was present only a short time in one compartment, until a definite response was made to the light, then the light was turned off. A period of delay was then started before the rat was released, a response appearing with the beginning of delay being the most significant thing in this experiment. Preliminary facilitation of direct progression to first one set then another set of compartments, prepared the way for the investigation of delayed reaction. No definite rotation in respect to the presence of light and the placing of food in a set of compartments was established, but different sets were lighted and they contained food in no regular order. Sometimes light and food were present in one set of compartments for two or three trials, particularly when progression was difficult to facilitate to one of the three of them. The object of this procedure was to facilitate direct progression with as great an ease to one set of compartments as to another. Because of asymmetrical development of the reflex extensor thrusts in most rats, direct progression could not be facilitated in one direction to a set of compartments as in another. A series of 10 trials daily was considered the most advantageous number to bring about the facilitation of progression in any of the three different directions to a set of compartments.

When a rat was placed in the release box, movements were made for escape to the outer and the larger compartment which led to one of the three food boxes. At this time, a slight delay occurred before the operator could raise the release box and free the rat. This delay was rather fortunate, for observations revealed that movements of parts of the rat's body were then made to facilitate progression when the rat was released. These movements were seen to be as significant as they proved to be in the entrance boxes in other problems, particularly in the mazes. In the early trials in the delayed reaction problem as in the maze problem, the facilitation of posture for progression began with the performance of a number of movements. At first posturing for direct progression was unattainable, but eventually the cumulative effect produced by all movements led to the development of interaction of body parts for posturing for progression. The visual stimulus from the light from a compartment, and other stimuli from the release box increased the number of reflex movements and activated many reflex parts of the organism.

Posturing within the release box was often not adequate for direct progression and reposturing outside of the release box was necessary before progression could continue to a compartment. The raising of the release box appeared to disrupt progression, making it necessary for posturing to take place a second time for progression. Conditions here were somewhat similar to the ending of progression in the early trials during the facilitation of posture for progression through turns in a maze. A disruption of progression at a turn took place in the same manner as disruptions of it following the raising of the release box. In both instances reposturing was more difficult when the reflex extensor thrusts were functionally undeveloped and reflex excitability was hyponormal.

After reposturing outside of the release box, progression was for some time diverted even into a non-lighted compartment. Direct progression to the lighted compartment required many trials before it could be facilitated, and in this respect it resembled the facilitation of responses in the preceding problem to two or to one of two lights of different physical intensities. In the early trials direct progression to one lighted compartment was as difficult as it was to two lights, or to one of two lights. In the preceding problem, it was necessary to facilitate posture to produce responses to one of two lights connected with the food; and in the delayed reaction problem, responses alone to one light in connection with food was required. Though in later trials in the preceding problem, there existed an increased difficulty to respond to two lights rather than to one, the presence of two lights required many trials before a response could be made to one light alone; in the delayed reaction problem, many responses to one light could be made, but because of the light occurring in any one of three positions, diverting of direct progression persisted for many trials. Posturing in a release box for direct progression in three different directions to a light is required.

Different degrees of posturing for progression in three different directions to the different compartments must be manifest when unequal antagonistic action of extension and flexion of the rat's limbs exists. As was seen in the maze problem, progression to the right is more difficult for rats than progression to the left and this is because of the predominance of the right neural center of the cord for extension and flexion of the right limb. Progression straight ahead to the compartment facing the front of the release box was at times more difficult than progression to the right or left hand compartment.

When 28 of 30 direct progressions were produced to the light, posturing for progression was thought to be facilitated. This number of direct progressions was more rapidly attained by some rats than by others. This difference in the time required to facilitate direct progression rested wholly upon the fluctuations in the unequal antagonistic action of extension and flexion of the rat's limbs. Fluctuations frequently were noted to produce divertings of progression, and sometimes these fluctuations produced several consecutive direct progressions to the light. Maximum divertings, however, appeared when reflex excitability became at any time hyponormal. In one or two instances when excitability was hyponormal, 10 daily trials could not be obtained. When the norm adopted had been reached, the problem of delay was instituted.

When consecutive direct progressions were facilitated, it was noted that in all the rats selected, the reflex thrusts had either disappeared or the extensor tone of the thrusts were very much weaker than in the beginning of learning. Only in one or two rats could a right or a left ipsilateral fore thrust be evoked when the position of the rat's head was changed in space. The extensor tone of the hind thrusts was weak, or these thrusts were absent, when the tail was dorsally extended. It appeared just as direct progression for several consecutive trials was about to be facilitated, the decrease in the extensor tone or the disappearance of the thrusts occurred. When these events had taken place, interaction of body parts for progression to a lighted compartment was developed.

Unfortunately at the time of the investigation of the delayed reaction problem, no rats with developed reflex extensor thrusts could be had. It is, however, not too much to assume from the results obtained with rats with functionally developed reflex thrusts when investigating learning in other problems, that the poor manifestations described above would not occur so frequently or persist so long in rats with well developed reflex thrusts as they have in the rats experimented with in the delayed reaction problem. Movements of progression can be performed by the rats selected with less facility than by rats with developed reflex thrusts, for the conditions that direct progression in one rat do so in all rats, irrespective of the development of the mechanism of extension and flexion of the rat's limbs.

Evidently the development of interaction of body parts is as essential a condition for the facilitation of direct progression in the delayed reaction problem as it was in other problems. The conditions which determined the development of interaction of body parts for progression before delay was adopted can not be different from those present when intentional periods of delay for immediate progression were started. It is apparent then, that the so-called delayed reaction problem is one in which there occurs an additional development of interaction of body parts for delayed progression to the source of a removed stimulus that was connected with the place of the food. Since biological changes are uniformitarian, learning must be likewise, and the changes produced in a rat as a result of instituting delay in progression, must be the same as those that originally facilitated direct progression with the exception that an enlargement of one phase or another occurs in the development of interaction of body parts for the facilitation of progression.

Two different methods were used to establish a delay for progression. These methods were tried not so much with any possible expectation that radically different results would be obtained, but to help observations of the movements of rats. With

4 rats, Hunter's method of turning the light off at different distances during progression to it, was first tried. The light was turned off first as the lighted compartment was neared, and then at different distances from the compartment. When this turning off of the light produced very little diverting of progression, the light was turned off at the time of the raising of the release box. Apparently this method, as instituted by Hunter, was to produce a gradual disruption of the "sensory associations" already formed, and to establish sensory excitations of another kind to produce activity. This method was soon discontinued when it was observed that the most important thing to consider was not a disruption of "sensory association" and the initiating of "something" to release activity when the external source of stimulation was removed, but the conditions that determined the behavior of the rats. A similar behavior was noted to appear when the foregoing method was used or when a new method was tried, namely, the turning off of the light simultaneously with the releasing of the rat after direct progression to the light was facilitated. With either method, progression was ended or diverted at the moment the light was turned off. There was immediately a return to behavior characteristic of the early trials of facilitation to a set of light and food compartments. Progression was often greatly diverted after it was ended outside of the release box and then continued. Sometimes it continued into a dark compartment. Similar ending and diverting of progression occurred when the latch-box was shifted or rotated from one position to another. Raising the release box simultaneously with the turning off of the light was continued until progression was frequently direct to the compartment containing food.

During the time of facilitating direct progression to the compartment in which the light was turned off, it was again noted that posturing for progression had once more to be facilitated. Posture was attained either by maintaining one position with the head directed toward the compartment or a series of forward movements were made, thereby attaining successive momentary postures. Scratching in one position in the release box was still another way of adjusting the body for progression. Of all these methods, a series of forward movements appeared to be the most effective. It was then clearly evident that the most significant thing to note was the facilitation of effective postures and not the number of "correct responses."

As was the case when attaining posture in other problems, the position of the different members of the rat's body in the attainment of posture is again of some interest. In the act of posturing, particularly when a series of forward movements are made, the rat's head is raised to the midline of the body, the tail may be extended and the extensor muscles of the trunk and abdomen are contracted. Again when one position of the body is maintained in a direct line with the previously lighted compartment, the same position of the head and body prevails. A different position is, however, attained when scratching is done; the head and the fore quarters are lowered, the hind legs are extended, raising the hind quarters. Contraction of many extensor muscles occur to attain posture for extension and flexion of the limbs for scratching which facilitates immediate progression when the release box is raised.

Only with the attainment of adequate posture was progression direct to the previously lighted compartment. Posture was often adequately attained in the left hand corner or the left hand side of the release box and seldom adequately attained in the right hand corner or side of the release box. Adequate posture in the left hand corner led more often to direct progression to the left hand set of compartments than to the compartments directly facing the release box or to the right hand set of compartments. Ineffective or inadequate posture produced diverted progression, sometimes direct to other compartments and more often to the compartments to the left. At times posturing was not observed to take place, and then progression was in no definite direction. This frequently occurred when reflex excitability was hyponormal. It may be said that diverting of progression to the previously lighted compartment varied from slight divertings from a direct course to wandering in any direction from the release box. When progression was greatly diverted, it was but one step to progression to another compartment. Progression was sometimes arhythmic. Sometimes retracing of steps appeared. Very often ineffective posture was seen to be due to fluctuations in the reflex thrusts as well as in reflex excitability.

The periods of delay for progression commenced usually with 1 second. The same behavior of rats existed for this period of delay as when the release box was raised simultaneously with the turning of the light. Experimentation with 1 second of delay was continued with 5 of the 11 rats used until a record had been obtained from 14 to 34 effective postures in 60 trials. With the remaining 6 rats 30 to 60 trials were given when the light was turned off with the raising of the release box, and when giving different periods of delay for progression. With the first group of rats gradual increases in delay from 1 to 3 seconds were tried, but it was soon seen there was no advantage gained in such an arrangement, and in the second group increases in delay varied from 1 to 8 seconds. It appears when an interaction of body parts for the attainment of posture for direct progression was facilitated, then increases of 5 seconds was most adequate to produce effective changes in the behavior of rats in the delayed progression problem.

The results obtained from experimentation on delayed progression are presented in table 43. In this table is found the number of trials required to facilitate direct progression to one or to another lighted and adjoining food compartments, the number of trials given when the light was turned off simultaneously with the raising of the release box, and the different periods of delay with the number of direct progressions in 30 to 60 trials. The first column under the heading of different periods of delay, gives the number of direct progressions, and the second column the number of trials in which these progressions occurred. The number of diverted progressions are not stated. These were more numerous in periods of delay from 1 to 5 seconds and then practically disappeared. Direct progressions to other compartments are also omitted.

This table shows that with an increase in the period for delay, a different behavior of the rats in the release box appears for direct progressions to the previously lighted compartment.

TABLE 43

NUMBER OF RATS	NUMBER OF TRIALS FOR DIRECT PROGRESSION TO LIGHT	NUMBER OF TRIALS WITH LIGHT TURNED OFF AT THE TIME OF RELEASE	DELAYED PROGRESSION IN SECONDS											
			11	2	3	4	8	10	14	20	25	30	40	50
1	180	270	15–60	12–60	9–30	19–30	13–30	11–30	16–30	13–30	16–30	13–30	13–30	8–20
2	180	300	18–60	27–60	7–30	11–30	8–30	5–30	9–30	10–30	8–30	10–30	9–30	
3	180	270	16–50	9–60	9–30	7–30	8–30	8–30	13–30	9–30	14–30	10–30	9–40	
4	170	240	14–60	15–60	10–30	10–30		8–30	11–30	9–30	12–30	6–30		
5	160	240	23–60	18–60	12–30	9–30		11–30	13–30	9–30	12–30	12–30	4–20	
6	330	430				7–30		8–30	7–30	12–30	12–30	3–30	3–30	
7	130	60	10–30		8–30		5–30	11–30	7–30	11–30	10–30			
8	240	30						4–30	9–30	5–30	11–30	9–20		
9	190	30	9–30					16–60	8–30	16–30	11–30	11–30		
10	180	60						11–60	9–30	10–30	14–30	6–30		
11	240	30	2–30		7–30			5–30	13–30	12–30	2–20			

With short periods of delay, approximately from 1 to 10 seconds, an effective or an ineffective posture in the release box, and a reposturing of the body after the release box is raised produces direct progression to the food. But with an increase in the periods of delay for progression, the behavior of the rat in the release box is different and diverted progressions practically disappear. Posture is more effectively attained inside of the release box, and in the majority of rats reposturing outside of the release box no longer occurs. With greater periods of delay the behavior of rats changes. A steady posture inside of the box can not however be maintained for any length of time and definite movements of another kind are made. More often after a series of forward movements have occurred, progression in any direction to the right or to the left continues until the release box is raised and then with increased speed it is directed to the food. Sometimes progression is only to the right or to the left, and not in both directions in the release box. Effective posture for progression once attained in the release box, is continued when this box is raised. It was also observed during progression in the release box, if another movement was made, such as raising on the haunches, posture for progression was disrupted and direct progression to the food did not take place. In other cases when a steady posture in one place in the release box or a steady posture for progression could not be maintained with increased delays, scratching in one particular spot occurred after one or more forward movements to the light were produced. Sometimes scratching followed progression in the release box. Often such movements as scratching were effective in maintaining posture for progression when the release box was raised. With long periods of delay, beyond 25 seconds, scratching to maintain posture was often ineffective. Interruptions of scratching then appeared and reposturing was difficult, though at times effective. At such times diverted progressions to the food reappeared. Uninterrupted movements of some kind were facilitated in the release box during long periods of delay.

Both scratching and progression result in the maintenance of rhythmic action of the limbs after a definite movement in the direction of the lighted compartment is made. With the continuation of rhythmic extension and flexion of the rat's limbs either by scratching or progression in the release box, prolonged periods of delay could be obtained up to 40 seconds, and in the case of one rat, up to 50 seconds. At these periods of delay, progression was continuously to the right hand set of compartments and, at times, inaction was the rule after a short time of activity in the release box. This inaction greatly increased the time between trials. An immediate response, the attainment of posture for progression did not not take place when the release box was raised. This absence of posturing for progression appeared not to be due to fatigue, as there appeared to be a limit to the period of delay in which effective responses or extension and flexion of the limbs in a confined place was possible. This ineffectiveness of rhythmic succession of limbs in a confined space for any length of time can be attributed to an impossibility of many parts of the body to interact to maintain posture for progression. At the end of 30 or 40 seconds, the functional condition of the rats closely resembled that of rats when the latch-box problem remained unsolved after successive daily trials of 30 minutes each. In both these instances, prolonged posturing to perform definite movements was impossible. In the delayed progression problem, posturing, so that the centers for progression in the cord could maintain effective rhythmic succession of the limbs for any great length of time in the release box is limited, or the limit of time for the persistence of an interaction of body parts for posture is dependent upon the functional condition of the antagonistic action of the rat's limbs and the mechanism for the production of reflex excitability. The condition of the thrusts determines the degree of posture possible and the presence of reflex excitability the rapidity with which posture is produced and its persistence. In rats in which an equal antagonistic action of the limbs existed and reflex excitability remained hypernormal better posturing would be produced, and undoubtedly the periods for delayed progression would exceed 50 seconds. The behavior of such rats when the limit to delayed progression was reached, probably would not result in inaction, but would reveal indications of posturing and of reposturing for progression in the release box. With well developed parts in rats developed interaction would be maintained for some time. Indications that continued interaction would be maintained in a functionally developed rat were repeatedly manifest in the rats used.

The problem of delayed progression requires the facilitation of posture for short periods of delay and the maintenance of progression, or scratching in the release box for longer periods of delay. When long periods of delay were given, progression was in some rats in different directions in the release box. When these conditions prevailed, it is apparently not necessary to make any distinction between a rat's movements and those of a raccoon or any quadruped animal. The facilitation of the mechanism of progression is more significant than the assumption of the establishment exclusively of a "sensory association" before delayed progression is started, when the latter is instituted, a greater facilitation of the same mechanism is of more scientific value than again assuming that the "kinaesthetic senses" function exclusively. The facts of physiology do not admit the proprioceptors functioning in any exclusive way and regulating and controlling movements. It must be admitted, however, that the conditions which determine delayed progression in animals can not be concerned in delayed progression in children, for in children, the greater freedom of eye movements and possibly language will initiate progression.

The most noticeable thing in the problem of delayed progression is the freedom of the action of the limbs, particularly the fore limbs, as is evidenced in scratching and in turning movements in the narrow confinement of the release box. Progression involves neural centers in the cord for extension and flexion of the limbs and associated centers. Progression being continued for 40 or 50 seconds in the release box, points to continuous activation of the centers in the cord in the way the centers for breathing are activated by the blood. This process is facilitated by the configuration of the inside of the release box leading gradually with increased delays to a greater development of interaction of body parts for progression. An extrinsic influence, light, produces posture for progression, and when the light is cut off, the configuration of the inside of the box permits an exaggerated continuation of posture for extension and flexion of the limbs in scratching or in progression, and when the release box is raised, progression is in the direction of the food.

(*To be continued*)

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A SUGGESTION AS TO THE MECHANISM OF MEMORY

GEORGE W. CRILE

I. THE MECHANISM OF RESPONSE TO STIMULATION

If we assume that the organism is an electrochemical mechanism, by means of which potential energy is transformed into kinetic energy in the form of heat, muscular action, and electricity, through the coördination of certain organs, then the following question naturally follows: How do stimuli which vary so greatly in their nature and in their intensity reach the brain through the vast numbers of delicate receptors of varying kinds, find their way through the intricate paths of the brain mechanism, and produce each its specific response? That is, how is it that light waves always activate the rods and cones of the retina; the coarser waves of sound the organs of Corti; the infinitely attenuated particles in the air the receptors in the nose? How is it that the activation of these receptor mechanisms, so delicately attuned to such infinitely small waves of motion and of chemical action can cause responses as powerful as those produced by the gross injury of tissue? A shell wound may cause no more activation than is caused by an intense emotion resulting from a danger that has been seen or heard. The variations in the intensity of the response to an adequate stimulus, the variations in the speed of the response, the relation of the response to variations in the force or in the area of contact of the adequate stimulus will be less difficult to understand if we consider them in the light of the action of certain man-made machines. A motor car may be started by the laborious process of cranking by hand, or by a light pressure on the starter button; a motor boat may be guided by a heavy unwieldy rudder, or by intangible wireless waves; the movements of Hammond's artificial dog, with its selenium eye, were governed by rays of light.

In the biologic mechanism, if it can be demonstrated that electric energy is released as a result of the application of the adequate stimulus, then these man-made mechanisms make it easy to believe that the brain can be activated to the utmost by infinitesimal stimuli.

We will grant that the man-made mechanisms are infinitely crude, as compared with the human mechanism, which has been evolved through aeons by infinite trials, resulting now in error, now in the survival of the evolving animal mechanism. Loeb (1) has shown approximately by what physical and chemical processes the rays of light orientate the simpler animals; how the light rays, acting like photographic processes, move animals adaptively. The approximate photochemical mechanism that produces motion toward colors as a means of protection is now known. Piper (2) has demonstrated in fish the presence of electricity in the auditory nerve as a consequence of sound waves. By a simple experiment, Steinach (3) has taken from the field of mystery the means by which the fish maintains equilibrium. Believing that the hair in the auditory sac was the receptor mechanism, he replaced the otolith by a piece of iron of similar shape. When a magnet was brought near this piece of iron, the iron was lifted into a new relation to the hair and in opposition to gravity. This caused the fish to execute bizarre movements, showing that the hair in the sac was in effect a self-starter, which when "pressed" by the otolith or the substituted iron, closed the circuit, the resultant electric current producing the muscular response. The fish had been adapted to gravity; magnetism suspended the law of gravity and upset the fish. The crude otolith of the fish in its exposed sac is replaced in the higher animals by the semicircular canals filled with fluid. This fluid, like the otolith, obeys the law of gravity; and it may be presumed that, like the otolith, it causes electric contact to be made and broken, thus releasing and activating the electric mechanism of the animal as do the buttons of the self-starters in man-made machines

Einthoven and Jolly (4) demonstrated that electric currents appear when a ray of light falls on the retina, and it is common experience that the application of electricity to the eye causes flashes of light. If the light rays falling on the retina cause electricity, and if electricity activates the mechanism by which the sensation of light is produced, then we may infer that light waves falling on this or that group of rods and cones may do for the mechanism of the brain, which responds to light by adaptive actions, what the sound-wave stimulation of the hair in the otolith does to the brain and the muscles of the fish; what the fluid in the semicircular canals does for the adaptive mechanisms of specific response in higher animals; what the hair in the Venus' Fly-Trap causes the plant to do to the fly; what the photoreceptive mechanism of the fish does to its adaptive behavior; what the selenium eye does to Hammond's artificial dog, or the wireless wave to the distant motor-boat. We infer that the ray of light broken by a shadow caused by the advancing enemy activates the motor mechanism of a soldier to fight or to escape through the agency of a mechanism no less adaptive than are the above mentioned mechanisms.

The hair of the Sensitive Plant, the hair in the otolith, the fluid in the semi-circular canals, the rods and cones in the eye, the photo-receptive mechanism in fish, are self-starters—they are selenium eyes of infinite fineness, and we may suppose that they are no less demonstrable and operate no less by physical laws than does the man-made device.

The conception that the brain mechanism is operated by electricity opens the way to illuminating interpretations of adaptive reactions. As Mathews (5) suggests, all cells are electric batteries. Hence, the large cells in the fundus of the eye, connected with the rods and cones, may be regarded as batteries, attuned to be discharged by the electric energy created by the action of the ray of light on the rods and cones. Nernst (6) first proposed, and many physical chemists have accepted the theory, that stimulation is not due to a continuous flow of electricity, but that interposing membranes must first be polarized by the accumulations of ions, stimulation taking place when a sufficient accumulation has occurred. If this theory be true, then a quantitative element is admitted so that we may suppose that the semipermeable membranes, in the case of the feeble electric current set up by a light wave, offer a correspondingly feeble resistance to be overcome before stimulation is achieved. Once the first cell in the path of the electric current is stimulated, and its electric charge is added, then the charges of the other cells lying along the base of the retina will be "fired" with great rapidity, augmenting the current. In this connection, it is at least interesting to note that the cells which are connected with

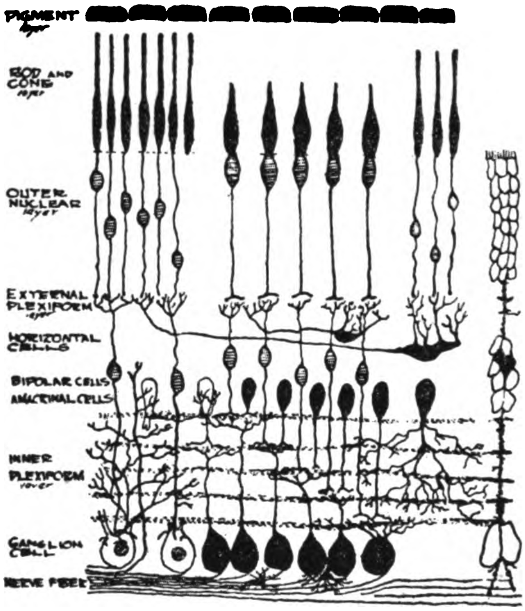

FIG. 1. STRUCTURE OF THE HUMAN RETINA
Adapted from Howell, Text Book of Physiology, 354 (1913)

the rods and cones are both *large* and *numerous* (fig. 1); whereas the nerve endings which act as receptors for physical injury, such as the sensory nerve endings in the skin, have almost no accumulators in the form of nerve cells to reinforce and augment their stimulus (fig. 2). The inference is that the infinitesimal receptor of the eye, which receives a beam of light of infinitesimal power, has made up for the want of initial physical force by adding a group of accelerating batteries. Were there a set of accumulators in the skin as powerful as those in the eye, endless explosions of energy would result. It is as important that the nerve receptors in the skin should have scant accelerating batteries to *minimize* the strength of the force in their specific stimuli as it is that the eye should have powerful accelerators to *augment* the infinitesimal physical force of its specific stimulus. It is of interest in this connection to note that Nissl found that the cells at the base of the retina became exhausted when the eye was long exposed to sunlight. Precisely similar changes, as we have shown, are found in the brain-cells generally as the result of a crushing traumatic injury. The blindness produced by sunlight is comparable to the loss of the power to produce body-heat, muscular work or mental action, which results from body-wide trauma. Body-wide prostration is traumatic shock; sun blindness is sunlight shock.

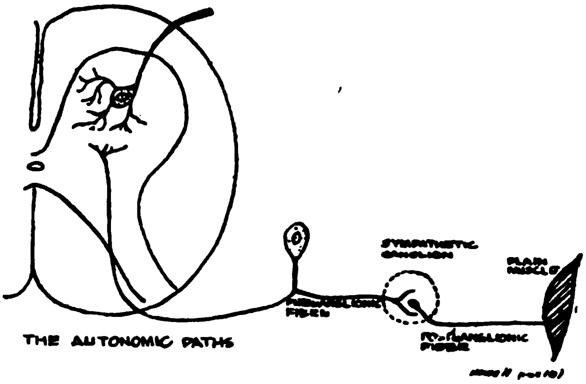

FIG. 2. SCHEMA ILLUSTRATING THE AUTONOMIC PATHS OF ACTION

Adapted from Howell, Text Book of Physiology, 140, 247 (1913)

A similar line of argument suggests the interpretation of the action of the auditory mechanism. As the passage of electricity through the eye causes the sensation of light, so the passage of electricity through the ear causes the sensation of sound. The organ of Corti bears a suggestive similarity to Helmholtz's resonators for the analysis of sound waves; and the added cell mechanism suggests an electric mechanism specifically adapted to respond to sound waves. Numerous nerve paths lead off from the auditory mechanism. The purpose of these, as of the cells which lie along the base of the retina may be interpreted by Nernst's law of augmentation or retardation of stimulation (fig. 3).

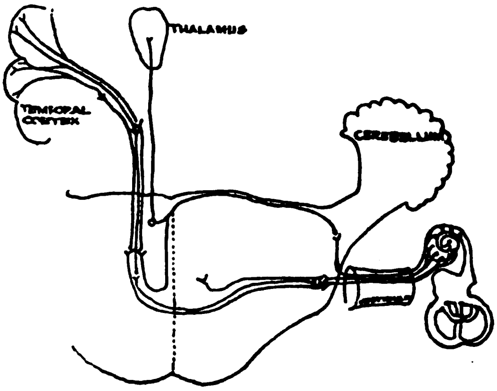

Fig. 3. Connections of the Auditory Nerve

Adapted from Piersol, Human Anatomy, 1258 (1907)

In like manner, the sensations of touch and pain, of pressure and distention, and the action of the various chemical and thermal receptor mechanisms may be interpreted. In short, no matter how slight the stimulus, or how delicate the mechanism, as long as it will inaugurate a current of electricity, however feeble, then the addition of accumulators can augment to any degree the force of its ultimate electric discharge. The brain cells supply the electro-motive force which operates the mechanism. The stepping up process may be similar to that of the *Audeon*. Thus the various types of receptors receive and transmit to the brain an infinite variety of electric currents from the various receptive mechanisms. These currents enter the brain on equal terms of competition for the possession of what Sherrington (7) calls the "final common path"—the path of action (fig. 4).

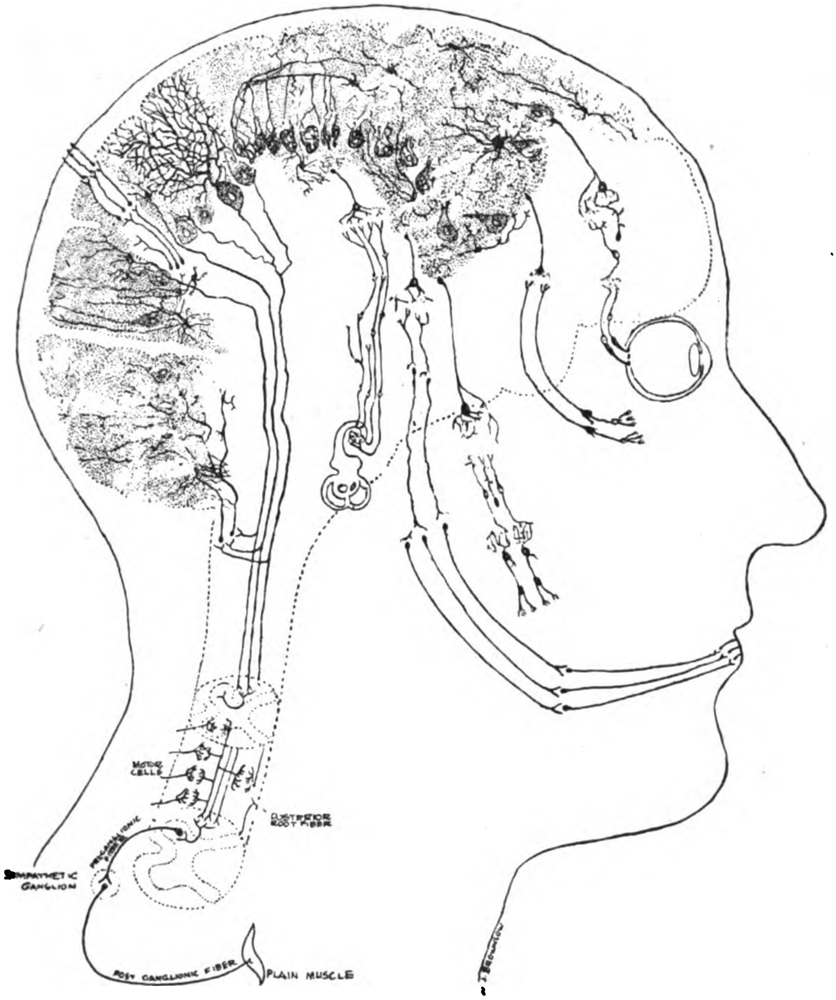

FIG. 4. SCHEMATIC DRAWING ILLUSTRATING THE VARIOUS MOTOR PATHS

II. THE MECHANISM OF SPECIFIC RESPONSE—ACTION PATTERNS

The preceding discussion suggests the manner in which the electric response to stimulation is achieved, but does not interpret the unfailing specificity of the response; does not explain how the myriads of electric discharges from the receptor mechanisms emerge as orderly action from the central organ of activation—the brain—with its infinite number of pathways and interlacings and apparent entanglements. The possibility of these ordered responses seems even more difficult of comprehension when we consider the fact that while the agency by which activation is secured is electric, yet the pathways in the brain are not insulated. Further consideration, however, will show that this fact in itself may possibly be the means by which both variety and orderliness of action are secured.

We will assume that the recording matrix does not include the brain-cells but only the non-cellular parts of the brain—the white matter. Are there any properties of this white matter that would suggest the nature of action patterns?

The white matter is a semi-fluid mass, 85 per cent being water.

The grey matter has a rich blood supply; the white matter has a relatively small blood supply.

In the grey matter, cells predominate; in the white matter, fibres and matrix-like substance predominate.

By subjecting both the grey and the white matter to stain for oxidase, Marinesco[1] showed that the grey matter is filled with oxidase, of which the white matter has none. The significance of this finding lies in the fact that the presence of oxidase implies metabolism and oxidation; it implies the *fabrication* of electric energy, not its *specific conduction.*

The grey matter is the working tissue. May not the white be the recording tissue?

The two sides of the spinal cord and of the brain have a crossed arrangement of the conducting paths. Paths from the left side cross over to the right; from the right side to the left; thus apparently complicating the mechanism.

This decussation could not have been evolved in the interests of economy, for it makes the paths longer, hence more material is required. It could not be to facilitate the rate of action, for the longer distance to be traversed requires more time. Its purpose could not be to secure a more certain blood supply, for no arteries cross. It could not be to secure coördination of the right and left sides; for in that case the responses to unequal stimuli would be the same. That it is an arrangement of the highest importance, we must assume, because it is universal in the higher animals. The brain and the cord are the only decussating organs; bones do not decussate; the heart does not decussate; blood-vessels do not decussate; the sympathetic nervous system does not decussate. Why does the cerebrospinal nervous system alone decussate? Let us examine further the kind of tissue that decussates. The white matter consists of highly specialized fats, among which is linolinic acid. Linolinic acid in itself may be supposed to have some interesting qualities in view of a certain property of linseed oil. It has been shown that linseed oil has the power to remember sunlight, and Mathews (8) states that linolinic acid probably has a higher development of the power of memory than linseed oil, which has even a capacity of a limited education in responding to light. Because of this property, Mathews offers an attractive speculation as to the possibility that memory may be the result of impressions made on the linolinic acid in the white matter. On the electrochemical basis, however, we may consider that the commissures, consisting of this white matrix, have been evolved through infinite trial and error as a mechanism for recording the *variations* in the magnetic field which accompany every passing specific electric current.

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[1] In the laboratory of Base Hospital No. 4 (The Lakeside Unit) in France.

The properties of the receptor mechanism in a wireless circuit are almost as wonderful as those in such a theoretic biologic mechanism. It is conceivable that thus the white matter develops a memory of this or that type of variation in the facilitation of the magnetic field resulting from variations in the incoming electric currents from the receptive mechanisms. Through association, therefore, the white matrix will respond in the same manner to a like facilitation from a receptor mechanism, even if days or months have elapsed since the facilitation was initiated.

As for the decussation of the white matrix, may its advantage lie in the possibility that by sending the action current from one side to the other the magnetic field is doubled; and in consequence a double impression is made on this matrix of infinitely impressionable material whose composition never changes during life, and whose constituents are almost unalterable and have no material metabolism?

We may thus conceive that the white matter functions as a phonograph matrix upon which each incoming stimulus has made its electrical record. When the brain-cells are again roused to action by a repetition of any one of the stimuli which has traced its original record, the outgoing electric impulses released by the stimulus traverse the facilitated path and reproduce the original action. A phonograph record will give back the same words or tune in after years. Why may not varieties of magnetic phenomena be written on the white matter, the recording tissue, and there await recall? But whether these facilitated paths consist of specifically altered pathways, or are the result of varying rates of vibration acting upon some mechanism similar to those which receive the varying rates of vibration which produce light and sound, we have no conjecture to offer.

Such a facilitation as we have suggested would be produced, whichever side of the brain received the initial or the repeated stimulus; for if the brain did not decussate, then if one first saw a rose with the right eye alone, the left eye being closed, it would follow that if the right eye were closed the rose would be a stranger to the left.

We may imagine that decussation pools the path of facilitating action, pools the incoming impulses and the outgoing currents of action, and produces like memories of the impressions received from either side—from either ear, either eye, either hand, etc. If there were but one eye, one ear, one hand, etc., there would be no need of decussation, no need that the right side should know what is done by the left.

As a consequence of these facilitated paths, whatever their physical mechanism, coördination of action results instead of chaos. Hence the stronger the impression made by a stimulus, the better will it compete for possession of the final common path—the path of action. The more frequently an act is performed, the deeper its impression on the white receiving and recording matrix. Hence, when an abnormally strong action current passes through the brain, it may facilitate a path so abnormally that it dominates all other impulses. Thus a financial or moral disaster, a great danger, a terrible scene may make such an impression—a path of such a degree of facilitation—upon the recording matrix that, from the moment it has been received, no other can compete with it, and the brain, in consequence, becomes approximately a one-path mechanism. Or a like over-whelming facilitation may be produced by the constant repetition of a single act—hence a dominating habit.

Thus, we venture to suggest that the white matter is the matrix on which the action patterns are written; that the cells of the brain supply the electro-motive force both for the magnetic field, variations in which may conceivably produce the facilitated paths of action, and for the resultant activation of the organism.

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THE PSYCHOLOGICAL SIGNIFICANCE OF BODILY RHYTHMS

WALTER M. COLEMAN

London

To test the effect of rhythm and the absence of rhythm on the mental states a beginning was made by studying the movements of animals under conditions of excitement, emotion, and of freedom from emotion. In order to check these results by subjective tests rhythmic and arhythmic human movements with their accompanying states of feeling were also studied. Rhythm is used here in the sense of periodicity, or regular repetition. In most cases the timing has been done by the simple expedient of starting a stop-watch many times at short intervals; for evidently the attachment and use of complicated recording apparatus would reduce the human subject or the animal to another state of mind and introduce new variables and sources of error that might defeat the object of the tests.

Periodicity cannot be accurately tested in these experiments by taking the time for half-minute periods. The custom has been inherited from physicians of taking the human pulse by timing it with the second hand of an ordinary watch for thirty seconds; but the beats may become faster or slower several times within the half minute and the liability to error is nearly one second, or about 3 per cent. But if a stop-watch is used the error does not usually exceed one-fifth of a second; hence the rate of the pulse (or footsteps, etc.) for six seconds can be taken with no greater liability to error than where it is taken for thirty seconds with an ordinary watch, and the observations can be repeated several times in thirty seconds so as to test for regularity as well as for average rate. Each method has been used below according to the experiment; where accurate proof of rhythm or lack of it was required the rate was taken for a few seconds only and the observation repeated a number of times in a minute.

When the pulse, steps, etc., are in accord the accompanying nervous impulses are of course likewise synchronous and any irradiations of nerve force from the several centers of activity will reinforce each other. When there are two or more periodic activities in discord their irradiations will interfere and, instead of a saving of energy, there will be a waste. I have made tests which indicate that rhythm is an essential factor in endurance and in the postponement of fatigue, a factor which seems to have been overlooked by students of that problem; but the question is beyond the strict scope of this paper.

Though the phenomena seem never to have been looked at from this aspect it was not surprising to find that the bodily rhythms were accordant in the mental states of calm, ease, steadiness, and sleep, and that they were discordant and imperfect during uneasiness, excitement, hurry, or emotion. The mental and physical states are probably inseparable and reciprocally causative.

Rhythmicity of the following activities were compared either simultaneously or successively:(1) the heart-beat and the footsteps; (2) the heart-beat and the breathing; (3)breathing and the steps or other bodily movements; (4) the heart-beat and attention to sensory stimuli.

Because of the integrative action of the nervous system, accord between a rate of stimulation and the several responses of the body is more general than discord. This makes for economical working, and indeed harmony seems to be an essential feature of sound living organisms.

Observations were first made upon animals in the zoölogical gardens in Regent's Park, London.

The tests had to be made in any order in which the activities of the animals presented opportunities; the first tests given here will concern *the relation between pulse and footsteps.* Test: A lynx, pacing to and fro in an outdoor cage was found to be taking 120 steps per minute; it stopped with its head up and its carotid artery throbbing in plain view. The count which was being repeated for its steps I was able to continue exactly on the pulse;

at the fifth beat of the pulse its foot struck the floor as it resumed walking. This occurred three times with pauses of varying length but no perceptible variation in rate. Test 2: A badger walking at 142 stopped and its heart-beat, plainly visible on its side, continued at the same rate. Test 3: The serval's heart-beat, plainly visible on its side, was 108; it started walking and the rate of its steps was found to be 108 (see also test 14). The heart-beat of the cheetah, leopards, seals, squirrels, and a few others is often visible. The light of an outdoor cage is usually needed; newly arrived animals are preferable for study. Animals dispirited by capture and by long confinement and also domesticated animals must be considered to a certain degree abnormal. Healthy animals born and raised in gardens make good subjects for experiment.

Of many tests of *the relation between the rates of heart and breathing* a few will be given. Test 4: cheetah, lying on its side with heart-beat plainly visible, rate 72. Its rate of breathing was 12 or one-fifth. Test 5: The movements of the whiskers of a resting leopard were perfectly regular and indicated its heart-rate as 54. The metronome was set at that rate with subdued sound, and expansions of lungs ceased at strokes of metronome as follows: 5, 5, 5, 6, 5, 5, 5, 6, 5, 5, 5, 6, 3. The animal stirred at 3. This and many other tests indicate that usually with animals at rest and always during sleep the breath begins at multiples of the heart-rate. Test 6: The grey mongoose; its carotid pulse was 180; its respiration, 90. Test 7: Bladder frog, throat breaths 60, with 1, 2, or 3 breaths omitted occasionally but the throat movements kept exactly on the count. Test 8: The heart beat of the sea-elephant, plainly visible on its side, was 80. The metronome was set with its heart; the giant seal then opened its nostrils at the following multiples of its heart beat: 7, 10, 10, 7, 9, 18, etc.

It appears also that there is often a *relation between respiration and movements of the body*. Test 9: An elephantine tortoise breathed once for each step. Test 10: A wolverine trotted at 17 steps in 7 minutes and, lying down, panted at the same rate. Similar tests were made with a badger; also with dogs in hot weather. Test 11: Fog on the breath of polar bears showed one breath for each step. The breath of draft horses was found by means of the fog on each breath on a cold morning to have an even ratio to their steps. Test 12: The tahr walked at 110; fog on its breath showed at every second step. Test 13: The ceradotus, or lung fish, breathed once to three movements of fins.

The respiration and even the heart-beat of many fish can be studied without interfering with the animal since the heart is on the floor of the throat. The pike is a good fish for the purpose: the soft skin at the base of the pectoral fins moves with the beat. When the fish is asleep the motion of this area and of the tongue reveal the heart-beat; for fish sleep with the gill-covers motionless and the mouth slightly open. Only methods of studying the pulse and breathing of animals without touching or disturbing them have been used; for man's power is felt so keenly by the lower animals that to handle them would bring in a confusing factor. Of course, when studying the reactions of so sensitive an organ as the heart, to mutilate or narcotize the animal might introduce error and perhaps render the results useless.

When an animal becomes inactive the motor nerve currents subside with the exception of cardiac and respiratory currents. The rhythm of respiration, when uncontrolled by those of the heart or limbs, seems to vary. When a resting animal resumes activity without suddenness or violence, and especially if there is no sudden change in its breathing, *the heart appears to act as pacemaker* and the cardiac nerve currents already established, set the rate for the new activity if it is of a nature to permit periodicity. Test 14: A man's pulse was 60. He was asked to walk at ease around a long table; his steps were counted and found also to be 60 per minute. This may be compared with test 3 above. The subject must not be told of the point that is being tested; as comparative tests show that, perhaps because of selfconsciousness, the subject is more likely to become either arhythmic or to change the rhythm. This experiment has been repeated with many subjects; sometimes instead of steps the arm has been flexed, etc. In a majority the action followed the rhythm of the heart.

When an action ceases and the same or another action is, after a short time, taken up at the same rate, this seems to be evidence that the *heart has acted as timekeeper and served to preserve the rate.* Test 15: A rhinoceros walked at 72. It rubbed its horn against a post at 72. Test 16: An ostrich trotted at 14 steps in five seconds. It walked at 7 in five minutes. The keeper startled it, it ran at 14 steps in five minutes; it ran much faster than it trotted but its steps (and heart?) were not hastened, its steps merely became much longer. Chimpanzees away from the forest and hippopotami out of the water are awkward animals, yet (test 17) a chimpanzee, using its arms as crutches, walked at 80; it walked on all fours at 160; it then chewed at 80. Test 18: The pigmy hippopotamus chewed at 60; it walked at 60. Test 19: The hooded crane walked at 80; it pecked grain from the ground at 160.

In one series of 300 tests of lower animals about 50 showed an *absence of rhythm.* Test 20: A caracal, expecting to be fed, walked at rates varying between 110 and 180, sometimes faster, sometimes slower. Test 21: A cheetah heard the keeper sharpening its knife; its steps varied within a few minutes between 180 and 220. Test 22: A leopard at 2 p.m. walked at 6 steps in 3.2, 3.2, 3.2, 3.3 seconds, but at 2.40, nearly feeding time, it walked at 6 in 2, 1.8, 2, 2.2 seconds. Test 23: Pere David's deer after shedding a horn breathed very irregularly. It appears that *emotive innervation from pain, desire, anxiety is accompanied by loss of rhythm.* Test 24: A Himalayan bear took 10 steps in 6 seconds; it was teased by a boy; it then took 10 steps in 6.2, 6.6, 5.8, 6.2, 6.6, 6.6, 6.6 seconds. It had recovered steadiness in two minutes but at another rate and perhaps with another tone of feeling. Test 25: 9.15 a.m.; of 30 animals 22 walked at a steady rate; 8 p.m.; of 30 animals none walked at a steady rate. This test can only be made on a sixpenny day with crowds that excite and fatigue the animals.

A number of unpublished experiments have agreed in pointing to emotion as a state of preparation for action by overbreathing or expanding and fixing the thorax for an accurate blow or spring, or by tightening the limbs on one side for the first step of flight. All these disturb the heart-rate for a time.

Darwin, writing of the expression of the emotions in men and animals, refers to emotional expression as belonging to rhythmical forms of activity. When emotion vents itself in primarily involuntary or free and relaxed forms of expression, as in laughing or sobbing, the action seems to follow either a simple or a compound rhythm; but, according to the foregoing records, when emotion passes, or is about to pass into voluntary action it disturbs the habitual bodily rhythms and causes periodicity to be lost.

The writer will probably find few to agree with his conclusions from experiments published elsewhere that equilateral right-left action is necessary for the preservation of rhythm, and that emotion usually produces unequilateral stress or strain preparatory for action. That unequal tension on the two sides makes the heart irregular may be tested as follows. Test 26: Subject, S. H., age twenty-four.

			Seconds		
1. Sitting, leaning against chairback; legs crossed	pulse, 6 in	4.8	4.8	4.8	4.8
2. Sitting erect with legs crossed	pulse, 6 in	4.8	5.0	4.4	4.6
3. Sitting erect, poised, balanced, feet and hands symmetrical	pulse, 6 in	5.2	5.2	5.2	5.2
4. Sitting as in 3, but reading a book	pulse, 6 in	4.8	5.4	5.2	4.0

Test 27: S., heart with systolic murmur; he usually sat unsymmetrical, head to one side.

			Seconds				
1. Sitting with legs crossed	pulse, 6 in	4.8	4.2	4.4	3.8		
2. Sitting poised; heads, hands, feet, symmetrical	pulse, 6 in	5.0	4.6	4.2	4.4	4.4	4.4

Similar tests were tried on 22 subjects with like results.

Observations were also made and experiments tried on *the effects upon the pulse of attention to periodic stimuli.* Test 28: The spotted daysure was walking at 156 steps per minute. A man came into the Small-Mammal House walking at 120 with loudly creaking boots. The daysure slowed and walked at 120. Test 29: Sheep grazing in a meadow sometimes assumed the rate of the footsteps of a passer-by on a gravelled footway.

Test 30: Four animals just before feeding time moved at rates 110, 170, 120, 136; a metronome was set clicking near their cages at 80; in ten minutes they were all walking at 160, and the pulse of one during a short pause was seen to continue at the rate of the steps.

Aside from the psychological bearings of the facts disclosed by the tests there is reason to think that they may have *a therapeutic import.*

1). On suddenly stopping after fast running there ensued a period of unpleasant palpitation and flushing. This was absent when the pace was gradually decreased. When the runner ran gradually into and out of the fast pace the exhaustion from the running seemed to be decidedly less.

2). One who always became breathless when halfway up a hill felt his pulse and began the climb breathing and stepping in unison with the pulse and climbed the hill without breathlessness, and the rise in blood pressure was only half as great.

The attempt to account for heart disease as chiefly due to valvular defects has been given up; perhaps the restoration of rhythmic habits may be of remedial value.

3). A subject with high blood pressure adopted the practice of counting his steps, "one, two" while inspiring, "three, four" while expiring for a few breaths whenever he began to walk. After three months his blood pressure had fallen by 20 mm. Hg.

Further experiments on the response of the heart to rhythmic acts and stimuli will be found in a paper by the writer, in the Journal of Psychology, liv, part III (in press).

The 30 experiments in this paper are a part of about 4000 tests made for the purpose of studying the effects of emotion upon animal behavior. It is not to be hoped that such pioneer experiments will prove unexceptionable from the point of view of technique; but it seems important not to neglect the study of will and emotion for the study of the learning process lest psychology, from the viewpoint of behavior, repeat the fatal course of the old experimental psychology and confine itself to a single aspect of mental action.

CONCLUSIONS

1. Muscular activity, both voluntary and involuntary, has a strong tendency to be periodic and the rhythms of the several activities (e.g., pulse, respiration, stepping, chewing) are usually in accord.

2. Although free emotional expression may be rhythmic, rhythms are brought to an end by suppressed emotional excitement, haste, or marked desire.

3. Attention to periodic sensory stimuli, in many instances, brings the bodily activities to the same rate.

4. A form of self-consciousness is brought on by discord of rhythms and tends to prevent return to harmony of rates.

5. The heart often serves as a time keeper and as a pacemaker.

6. Rhythm and arhythm induce entirely different states of feeling and are factors in the postponement and onset of fatigue and altered cardiac tension.

7. The less apparatus used the greater will be the probability of reliable results and the greater the freedom for new forms of experiment.

INTEGRATION OF MOVEMENTS IN LEARNING IN THE ALBINO RAT

A STUDY OF THE ADJUSTMENT OF AN ORGANISM TO AN ENVIRONMENT

JOHN LINCK ULRICH

VIII. THE PERMANENCY OF INTERACTION OF BODY PARTS AFTER A PERIOD OF REST OR REDINTEGRATION

No investigation of learning is quite complete without a consideration of the permanency of developed interaction of body parts after a problem has been learned, and the institution of a period of rest. After a period of rest, relearning is often required, and the changes that have taken place in an organism which necessitate relearning have been most often regarded as retrogressive. These changes have been explained by saying there has taken place a loss in retention, or less often in animal psychology, a loss in memory. Recently Brockbank (51) has shown that a better conception of this loss can be had by saying that the problem of retention is one of redintegration, which implies a complete or partial recurrence, after a period of rest, of movements that appeared during learning. A "loss" in redintegration after a period of rest can imply nothing more than a change in the coördination of integrated movements. It moreover signifies, when a change is evident, that learning was not complete at the time the rest period began; or can never become complete. This incompletion of learning is indicated with the recurrence in the redintegration series, or during relearning, of the imperfect responses that appeared during learning. Something then has been more definitely expressed when using the term redintegration than when using either of the terms "retention" or "memory." These terms do not express the functional condition of the organism, that is implied by the term redintegration.

If the term "retention" is an inadequate one to express the conditions that are observed to take place after a period of rest, much more so is the expression a loss in retention, particularly so when it is implied by this expression that retrogressive changes have occurred. A "loss in retention" has been variously and at times only incidentally referred to. This loss is sometimes said to be due to a loss of a sensory cue, or to an extraneous disturbance. Olfactory stimuli have been regarded as producing the greatest disturbances, for often when an animal is reintroduced into the problem, after a period of rest, evidence of timidity is noted. Such explanations are quite in common with theories of learning based upon "sensory association." But sometimes relearning reveals that much more is apparently lost than can be accounted for by a loss of a sensory cue conditioning a disruption of "sensory association." Then this fact has been explained by saying that a "loss in retention" is to be attributed to retrogressive changes which have taken place in the nervous system or nerve cells during the period of rest. It is usually said that the changes that occur at this time have destroyed a previously established organization, or impressions brought about by learning, and on a return to practice, the retrogressive changes are checked and the progressive kind suitable to learning are restored. Relearning is accomplished with greater ease than learning, for the retrogressive changes in the nervous system have not been completely destroyed.

These retrogressive changes that are said to occur in the nervous system are far too uncertain for any serious consideration in a "loss in retention." Most theories of learning totally ignore the possibility of retrogressive changes occurring in the anatomical structures which are supposed to be directly concerned in learning. They state that during learning either synaptic membranes become more permeable to sensory excitations for the production of the "successful" movements, that the performance of these movements through definite sensory excitations firmly sets into action certain neural arcs to the exclusion of others, or that reintegration in the nervous system takes place. After a period of rest, these synaptic membranes should become less permeable, or the neural arcs less selective in their action or a disruption of reintegration occurs, but such retrogressive changes are seldom mentioned and a loss is more often attributed to a disruption in "sensory association" established during learning, or to an extraneous disturbance. Logically these changes should be supposed to take place in the synapses, in the neural arcs, or in the nervous system and not in a loss in sensory association, for these structures ought to be physiologically more directly concerned in establishing "sensory associations" than the external senses. Relearning has ever indicated that much more must be regained than a reëstablishment of "sensory association."

Again the belief in the existence of an hypothetical factor, such as pleasure or sensory intensity in animal learning, producing with a repeated performance of the "successful" movements, a fixation of these movements, has not in previous work on retention been made an important factor. Pleasure can not be effective in the fixation of movements unless it becomes significant as a protreptical tendency. This tendency must again be present when after a period of rest a rat is placed once more in the problem. If a perceptible loss in "sensory association" exists at the time of the retention test, then a corresponding loss in this tendency must also be apparent. The loss in retention can not be primarily one of sensory association, for both factors, pleasure and sensory intensity, are supposed to be practically one with sense functioning. Such considerations are too indefinite in their application to well founded facts obtained during relearning, and can be conveniently dispensed with in problems of redintegration.

A brief consideration of the interaction of mechanistic parts necessary for learning, will point to conditions which determine redintegration. In the present investigation of the conditions which determine redintegration, these mechanistic parts and the fluctuations that take place in them must be carefully considered, for any fluctuation in them must necessarily disrupt the developed interaction of reflex parts facilitated during learning. It is also conceivable that metabolic changes during a period of rest might be considerable factors determining redintegration.

Redintegration tests were made with rats with the latch-box, the inclined-plane, and the circular maze problems. These three problems were selected, for each reveals different uses of the mechanism of the reflex thrusts. One important phase in learning was seen better in one problem than in another, and the same thing was observable in redintegration. In the latch-box problem, the appearance of the facilitation of the coördination of integrated reflexes for the attainment of posture was significant in the investigation of the persistence of developed interaction of body parts of an animal for learning. The same thing was observed in the inclined-plane problem for the persistence of one method and the effective performance of the extensor thrust to plunge the plane. The persistence of developed interaction to attain posture for the learning of the maze problem was revealed in direct progression through consecutive alleys and turns to the goal. The effective performance of coörodinate movements indicates the persistence of interaction of body parts, and the ineffective performance of these movements a disruption of interaction. Both the effective and ineffective performances of movements must be a fundamental consideration in the investigation of redintegration.

With the latch-box problem 15 rats were used. These rats possessed undeveloped reflex thrusts, or an unequal antagonistic action of extension and flexion of the limbs. Fluctuation in the thrusts occurred from time to time. In one or two rats fluctuations in reflex excitability were also manifest during learning. Experimentation began when the rats were 53 days old. Training was continued until 13 out of 15 perfect records were obtained. The one trial a day method was used during learning and the redintegration tests. A rest period of 70 days intervened between the last trial of learning and the first of the redintegration series. During the rest period, the rats remained in the living cage and were fed once a day. Sufficient food was given to maintain healthy metabolism and growth without the taking on of fat.

The most noticeable behavior on the first redintegration test is the slowness of responses in rats manifesting hyponormal reflex excitability in comparison with the last trial of the learning series. This slowness in many cases seems to be due to the employment of many more muscles which is evident in a slight crouching of the body, particularly in the entrance box to the hood. Sometimes timidity accompanies crouching and the slowness of responses. With the slowness of responses directive integration does not take place, but progression is to one side of the problem or around it. When reflex excitability is greatly hyponormal, wandering progression without manifestations of crouching or timidity is evidenced. Not until the door of the problem is touched by the snout or vibrissae, or vision produces responses to the door or latch, is posture attained and the latch raised. Occasionally the first productions or attainments of posture are ineffective and two successive attainments of posture with the production of two neck reflex movements occur, or after the first ineffective attainment of posture, progression occurs around the problem. In the last instance, there is an increase in speed of progression, and in the performance of movements to solve the latch-box problem. With the employment of many integrated reflexes at the time of posturing, there is an increase in the manifestation of reflex excitability. In all the instances noted above, a number of movements must be performed before reflex excitability reappears and effective rapid movements are made. All this behavior indicates that there is a return to conditions observed during learning, in particular during the early trials.

On the other hand with rats manifesting hypernormal reflex excitability, the usual movements made in the entrance box to attain posture for direct progression to the door of the problem box soon appear. When the door is opened, direct progression to the door or even to the position under the latch occurs. Sometimes progression is perceptibly slower than the last trial of learning. To attain posture under the latch to produce the neck reflex movement is again often difficult as was repeatedly the case during learning. The occurrence of imperfect responses, ineffective attainment of posture for the production of the neck reflex, indicate that a disruption of developed interaction of body parts facilitated during learning has taken place. Only in one rat was directive integration and the attainment of posture to produce the neck reflex movement effectively performed on the first trial of the redintegration series and no indication of a disruption of developed interaction was apparent. After the first trial, 50 perfect records were obtained.

The first trial of the redintegration series shows that a great difference in the effective performance of movements exists in rats, and these are noticeable in the entrance box. Previously it has been shown that not alone must posture be facilitated in the entrance box for direct progression to the problem box, but it also must be maintained. Posturing was not alone required in the entrance box of the maze for direct progression in the first alley, but also in the problem of delayed progression. A form of progression must also be facilitated in the entrance box for the attainment of direct progression when the rat is released. Posture in a crouched position is ineffectively attained in the entrance box in rats manifesting hyponormal reflex excitability, and it is most readily attained when reflex excitability is hypernormal. This observable difference in the attainment of posture appears to be due to the greater facility with which the reflex parts of rats work together when excitability is hypernormal than when hyponormal. This working together of certain parts of the rat's body may even take place in those rats which manifest excitability prior to being placed in the entrance box or immediately thereafter, for struggling to be free is frequently manifest. This fact is in accord with the statement of Bainbridge (52) that before exercise under stress of excitability neural propagations from higher centers to the medullary centers bring about a rise in blood pressure, deep breathing and greater frequency of the pulse; and the blood supply to the heart, to the brain, and to the muscles is thereby at once increased. All these changes as previously stated increase the extensor tone of the integrated reflexes especially required for coördination for learning. Since this coördination of integrated reflexes is maintained by interaction of many body parts, a disruption of interaction would most likely occur in rats manifesting hyponormal reflex excitability. Reflex excitability is then, one of the determining things which condition perfect as well as imperfect records on the first trial of the redintegration series.

Other changes beside reflex excitability becoming hyponormal condition perfect records in the redintegration series; for often when reflex excitability is hypernormal, the attainment of posture under the latch to produce the neck reflex movement is for a time difficult. These changes become clearly evident when several trials have been given. Again, fluctuations in the unequal antagonistic action of extension and flexion of the rat's limbs increase from time to time the difficulty to attain posture. After a rest period, a decided fluctuation in the reflex extensor thrusts were noted to produce marked changes in movements on the first and succeeding trials of the redintegration series. These observations will be considered more in detail when redintegration in the inclined-plane and circular maze are presented.

Redintegration trials with the latch-box were continued until a total of 50 trials were given. A continuation of trials was undertaken to observe the possible improvement in the development of interaction since relearning is usually more rapid than learning. Long before the end of 50 trials, interaction of body parts for the attainment of posture was seen to be greatly improved. An improvement was evident in the reduction in the number of fluctuations in the extensor thrusts and in the disappearance of imperfect responses. Again fluctuations in the extensor thrusts at the end of learning appeared at times to produce imperfect responses. These fluctuations were less effective in producing these responses during the redintegration series. In most rats after a rest period of 70 days, the interaction of body parts for learning was improved.

This improvement in responses with the addition of trials is of some physiological interest in connection with learning and with redintegration. It is absolutely essential that in learning movements of the muscles on the one hand, and activities of circulation, of respiration, and of the mechanism for the production of reflex excitability on the other hand should interact as a harmonious whole. Perfect coördination of integrated reflexes is thereby more likely to be facilitated, even though the extensor thrusts are undeveloped in rats. A disruption in interaction of many body parts as is evidenced by reflex excitability becoming hyponormal, is less likely to occur, and there is less evidence that fluctuations in the extensor thrusts produce imperfect responses. The fluctuations may tend to disappear. It is evident that only by constant training can developed interaction be maintained. During any period of rest, fluctuations in reflex mechanisms are prone to occur.

The investigations of redintegration in the latch-box problem were preliminary to a more systematic investigation of redintegration in the inclined-plane and the circular maze problems. A more definite procedure was possible when some of the conditions which determine redintegration were known. The results obtained from the inclined-plane and the maze problems during learning will aid in an interpretation of the redintegration series of trials with these problems. The results obtained from the learning series of the problems were very different, and like results from the redintegration tests are to be expected.

With both the inclined-plane and the maze problems, the same experimental methods were used as with the latch-box. All rats were 42 days old when learning began. One trial a day method was used during the learning and the redintegration series. Rats were divided into two groups; one group was given a rest period of 30 days and the other of 75 days after the problems were learned. Different periods of rest were given, since it is supposed that more evident changes in an organism take place after a longer period of rest than after a shorter one followed by the continuation of training.

In the inclined-plane problem, a total of 20 rats were used. Learning was continued for 60 trials. This number of trials was given because very few consecutive perfect records were obtained, and often the greatest number of perfect records were made before this number was reached. But the object of giving this number of trials was to facilitate one constant method to plunge the plane and to determine the persistence of a method after a period of rest. The extent of the loss in developed interaction of body parts for learning can be observed in the persistence of one method to plunge the plane. Of the rats used 10 were given after 60 trials, a rest period of 30 days, and 10 rats a rest period of 75 days. The rats in each group showed different degrees of development of the extensor thrusts, and of the mechanism for the production of reflex excitability.

The first trial of the redintegration reveals more clearly than other trials any change that has taken place in the rat's body parts, for often a few trials may obliterate all evidence of change. A restoration of developed interaction of body parts may appear on the second trial or after a few succeeding trials. Some of the changes that were manifest are indicated on tables. The first trial R. is contrasted in tables 44 and 45 with the last trial of learning, L. The first of these tables gives the records of 8 rats after a rest period of 30 days, and the second table the record of the same number of rats after a rest period of 75 days. Both the first and second lines of each rat's record give the reaction time for solving the problem, the condition of directive integration, the condition of reflex extensor thrusts, the direction of progression to the plane, the degree of manifestation of reflex excitability and the method used to plunge the plane. The record of the degree of reflex excitability manifest is that of the first trial of learning and the first of the redintegration series. The degree manifest on the last trial of learning was invariably hypernormal and it was thought advisable to give instead the degree of reflex excitability shown on the first trial of learning. Though reflex excitability at times in a few rats becomes hyponormal throughout learning, it did not become hyponormal in the 16 rats on the last of the sixtieth trial of learning. A perfect record is indicated by an asterisk.

Because of the irregularity with which perfect records were produced in the inclined-plane problem, it is impossible to judge from this table the best condition of redintegration in rats. A perfect record may be produced on the last trial of learning and may not occur on the first trial of the redintegration and vice versa. Only in one rat were perfect records made on these two trials. Other factors, however, are of great interest, since the conditions prevailing during learning may or may not be the

TABLE 44

Rest period 30 days

NUMBER		TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
		To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	R.	14	1	15	Direct	Direct	Right	Left	Right wk.	Weak	Hypernormal	M. T.
	L.	4	1	5	Direct	Direct	Right	None	Right wk.	Strong	Hypernormal	M. T.
2	R.	8	4	12			Right	Right str.	None	Strong	Hyponormal	Step
	L.	5	1	6	Direct	Direct	Left	Right str.	Right wk.	Weak	Hyponormal	Step
3	R.	7	2	9	Direct	Direct	Left	Both wk.	None	Strong	Hypernormal	Step
	L.	1	1	2	Direct	Direct	Left	None	None	Strong	Timid	Step
	R.	7	2	9		Direct	Right	None	None	Strong	Hyponormal	M. T.
	L.	6	2	8	Direct	Direct	Right	Right wk.	None	Strong	Hyponormal	M. T.
5	R.	11	1	12	Direct	Direct	Right	Both	None	Weak	Hypernormal	Step
	L.	5	1	6	Direct	Direct	Right	Both	None	Strong	Hypernormal	Step
6	R.	5	1	6	Direct	Direct	Right	Both.	None	Strong	Hypernormal	Step
	L.	1	1	2	Direct	Direct	Right	Right	Right	Strong	Hypernormal	Step
7	R.	10	2	12	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
	L.	11	1	12	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
8	R.	2	5	7	Direct	Direct	Right	Both	Both	Strong	Hypernormal	Thrust
	L.	2	1	3	Direct	Direct	Right	Both	Both	Strong	Hypernormal	Thrust

TABLE 45

Rest period of 70 days

NUMBER	TIME				DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
		To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	R.	1	1	14	Direct	Direct	Right	None	None	None	Hypernormal	Step
	L.	17	2	19	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
2	R.	83	2	85	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
	L.	6	1	7	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
3	R.	4	1	5	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
	L.	1	1	2	Direct	Direct	Right	None	None	Weak	Hypernormal	Step
4	R.	118	11	128				Both wk.	Right wk.	Strong	Hyponormal	Step
	L.	1	1	2	Direct	Direct	Left	None	Right wk.	Strong	Hyponormal	Step
5	R.	3	2	2	Direct	Direct	Right	Both	None	Weak	Hypernormal	Step*
	L.	6	1	7	Direct	Direct	Right	Right	None	Weak	Hypernormal	Step*
6	R.	4	1	5	Direct	Direct	Right	Right	None	Weak	Hypernormal	Step*
	L.	61	8	69	Direct	Direct	Left	None	None	Weak	Hypernormal	Step*
7	R.	61	8	69				None	None	None	Hyponormal	M. T.
	L.	5	1	6	Direct	Direct	Left	Both	None	None	Hyponormal	M. T.
8	R.	11	2	13				None	None	None	Hypernormal	M. T.
	L.	4	1	5	Direct	Direct	Right	Right	Right	Strong	Hypernormal	M. T.

same as on the first redintegration test. It will be noted on table 45 that when a rest period of 75 days was given, the reaction time for the first trial of the redintegration series usually averaged higher than for the same trial after a rest period of 30 days. This increase in reaction time after 75 days rest indicates a change in the developed interaction of body parts. A fluctuation in the reflex extensor thrusts can be seen to have produced some change in the responses of rats. When fluctuations occurred, the direction of progression to the plane was in two rats changed. This change in direction of progression is to be seen in the record of rat 2, table 44 and rat 6, table 45. When investigating learning of the inclined-plane problem, fluctuations in the extensor thrusts conditioned a change in the direction of progression to the plane and the same thing occurs in redintegration. A more decided change was evident when reflex excitability was hyponormal on the first trial of the redintegration series. When reflex excitability was hyponormal on the first trial of learning, it was again hyponormal on the first trial of the redintegration test, and in both trials the same behavior in rats occurred. Wandering progression appeared before the plane was plunged and usually during progression to the opened door. In rat 4, table 44, direct progression to the opened door took place, it did not appear direct to the plane, and it was established in the same way as in learning. In rat 4, table 45, reflex excitability was decidedly hyponormal on the redintegration test and no response to any part of the problem appeared until the plane was touched by the fore feet. Progression was slow and undirected until the plane was touched when it was directed more often to the plane. There is in this behavior a return to conditions that prevailed in the early trials of learning. This statement indicates, that like the reflex extensor thrusts, the mechanism for the production of reflex excitability can not be developed by training.

One condition which determines the persistence of developed interaction of body parts for learning is the constancy of reflex excitability. When reflex excitability becomes hyponormal then disruptions of responses follow. Fluctuations of this kind have occasionally been noted to produce during learning similar results, but they are much more pronounced after a period of rest. It appears that in rats in which reflex excitability becomes hyponormal, only through constant training can develop interaction of body parts for learning be satisfactorily maintained. It is scarcely possible that for rats, with a defective mechanism for the production of reflex excitability, redintegration trials can be perfect.

Though directive integration is disrupted when reflex excitability becomes hyponormal, one method in general to plunge the plane persists. The persistence of one method, stepping on the plane, the producing of an effective extensor thrust, or of a modified extensor thrust, indicates once more that the method used is determined by the development of the extensor thrust and that improvements in these thrusts when they are undeveloped does not take place. It is possible, if improvement did take place in the extensor thrusts, and a more effective method than stepping on the plane was used, then changes in method used to plunge the plane might appear. Temporary changes in method at times do occur, not from any improvement in the thrusts, but from fluctuations in the unequal antagonistic action of them increasing or decreasing their extensor tone.

The recurrence of fluctuations in the extensor thrusts condition disruptions in interaction of body parts with the production of imperfect responses in redintegration as in learning. When fluctuations in the extensor thrusts are frequent in the early part of learning, they reappear with almost the same frequency in the early redintegration series of trials and they may even be more pronounced during the redintegration series. These fluctuations can be seen on tables 46, 47 and 48 of redintegration. The learning series of these redintegration trials are presented on tables 12, 13 and 15. A rest period of 75 days intervened between the last trial of learning and the first trial of the redintegration series. Since no improvement in relearning over the learning series was evident, only from 14 to 15 trials of the redintegration trials were given.

TABLE 46

Redintegration of learning table 12

NUMBER	TIME			DIRECTIVE INTEGRATION		REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To door	To plane	Total	To plane	To door	Fore	45 degrees	Hind		
1	2	5	7	Direct	Direct	Both	Right	Strong	Normal	Thrust
2	9	1	10	Direct	Direct	Both	Right	Strong	Normal	Thrust
3	12	1	13	Direct	Direct	Right str.	Right	Strong	Timid	Thrust
4	5	1	6	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
5	3	1	4	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust *
6	2	1	4	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
7	15	1	16	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
8	4	1	5	Direct	Direct	Right str.	None	Strong	Hypernormal	Thrust *
9	2	1	3	Direct	Direct	Right str.	None	Strong	Hypernormal	Thrust *
10	60	1	61	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
11	21	1	22	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
12	11	2	13	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
13	5	1	6	Direct	Direct	Right and cont. left	None	Strong	Hypernormal	Thrust
14	5	1	6	Direct	Direct	Both	None	Strong	Hypernormal	Thrust
15	3	1	4	Direct	Direct	Both	None	Strong	Hypernormal	Thrust

* Direction to the plane to the right.

TABLE 47

Redintegration of learning table 13

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	10	2	12	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
2	9	1	10	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
3	4	1	5	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
4	3	1	4	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
5	3	1	4	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
6	3	1	4	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
7	5	1	6	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
8	3	1	4	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
9	5	1	6	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
10	5	1	6	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
11	6	1	7	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
12	4	1	5	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
13	4	1	5	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
14	6	1	7	Direct	Direct	Right	None	None	Strong	Hypernormal	Step
15	4	1	5	Direct	Direct	Right	None	None	Strong	Hypernormal	Step

TABLE 48

Redintegration of learning table 15

NUMBER	TIME			DIRECTIVE INTEGRATION		DIRECTION TO PLANE	REFLEX THRUSTS			REFLEX EXCITABILITY	METHOD
	To plane	To door	Total	To plane	To door		Fore	45 degrees	Hind		
1	61	5	66				None	None	None	Hyponormal	M. T.
2	23	2	25			Right	Left	None	None	Hyponormal	M. T.
3	59	1	60		Direct	Right	None	None	None	Hyponormal	M. T.
4	4	1	5	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
5	4	1	5	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
6	9	1	10	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
7	8	1	9	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
8	4	1	5	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
9	12	1	13	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
10	2	2	4	Direct		Right	None	None	Weak	Hypernormal	M. T.
11	3	1	4	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
12	5	1	6	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
13	10	2	12	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.
14	7	1	8	Direct	Direct	Right	None	None	Weak	Hypernormal	M. T.

Table 46 is the redintegration series of the learning table 12. This last table shows that the reflex thrusts were fairly well developed, but unequal antagonistic action of the limbs was manifest. Accordingly, many consecutive perfect records were obtained. On the fifty-ninth trial, a decided fluctuation in the reflex thrusts occurred with the appearance of a contralateral left ipsilateral thrust synchronously with the right ipsilateral thrust. The same change in the reflex thrusts appeared more frequently during the redintegration series, and profoundly affected the results obtained for very few perfect records were made. Many imperfect responses occurred, particularly before the plane; as many as three ineffective extensor thrusts were repeatedly produced before the plane was plunged. Movements were slower, and progression to the plane was at times greatly diverted. A change in the functional condition of the reflex thrusts during a rest period evidently produced an entirely different record from that of the learning series.

Table 47 gives the redintegration trials of the learning table 13. A comparison of these two tables shows that the reflex thrusts on both are indicated as undeveloped. The learning table shows that for 41 trials no fore limb reflex thrusts were evoked and only occasionally the hind limb thrusts, and the same thing was revealed after a rest period of 75 days. The redintegration tests revealed practically the same conditions that prevailed during learning. The reaction time of the first trial of the redintegration series was not greater than the reaction time of some of the last trials of the learning series. Progression to and from the plane was as direct as in the learning series. The learning table shows only three isolated perfect records out of 60 trials and the redintegration none in 15 trials. The same method of plunging the plane, namely, running and stepping on it prevailed after a 75 day rest period. It appears if the reflex thrusts remain constant and reflex excitability is manifest, redintegration varies little from learning.

The most marked changes after a period of rest, occur when the reflex mechanisms greatly fluctuate. These changes can be seen on table 48, which gives the redintegration trials of the learning trials of table 15. Both the tables for learning and for redintegration show that fluctuations in the reflex thrusts and in reflex excitability occurred. For three trials of the redintegration series, reflex excitability was hyponormal, as it was in the early trials of learning. Throughout learning either a right or both ipsilateral fore thrusts were evoked when the rat's head was bent to the right or to the left of the midline of the body, and during the redintegration trials, the left ipsilateral fore thrust or no extensor thrusts were evoked. Other reflex thrusts, those of the fore limbs when the head was dorsally extended 45 degrees, were not evoked during the learning and the redintegration series, and those of the hind limbs when the tail was raised were generally weak or absent. As a result of these fluctuations in the rat's reflex mechanisms, progression to and from the plane was not refacilitated until the fourth trial. Progression was slow, in many directions, and a response to the plane did not take place until the rat's body touched the plane. During the 15 trials of redintegration, not one perfect record was made.

From these three different tables, representing conditions typical of the redintegration in the inclined-plane problem, it is evident that retrogressive changes during a period of rest do not take place, but there is again manifest, difficulties observed to have been present during learning. Any change that is apparent in redintegration seems to be due to a natural fluctuation in the reflex extensor thrusts or in the mechanism for the production of reflex excitability. Fluctuations in these mechanisms are much more evident in some rats than in others. In the record presented on table 47 of redintegration, no evidence of fluctuations appears and no so-called "loss in retention" is noted. On the other hand, the other two tables show that fluctuations take place and redintegration as a consequence is poor. During a period of rest, fluctuations are not accelerated, but the usual natural fluctuations in the rat's reflex mechanism occur. As previously stated, only by constant training can the efficiency of the organism be maintained when the reflex thrusts fluctuate or when the mechanism for the production of reflex excitability is undeveloped.

The results obtained from investigation of redintegration in the inclined-plane problem show that when fluctuations of the reflex thrusts occur during learning, they appear in the redintegration. These responses are the same if the fluctuations are not very marked during or after a period of rest, but are quite different if decided fluctuations occur in the fore ipsilateral thrusts. In addition an absence of these imperfect responses at the time of redintegration and during learning indicates the efficient manner in which directive integration is maintained and in which the method used is performed. On the performance of these two movements in redintegration more information is desirable; but little additional knowledge of these two movements can be had from additional experimentation with the inclined-plane problem. So infrequently were consecutive perfect records made with the inclined-plane problem, that it was essential to investigate redintegration in the maze problem where often many consecutive perfect records are produced. The maze problem, in many respects, appears to be the best for investigation of redintegration, for every imperfect response can be seen to be conditioned by a fluctuation in the unequal antagonistic action of extension and flexion of the limbs.

Two different groups of rats were used in the investigation of redintegration in the circular maze problem. One group of 11 rats was given a rest period of 25 days and another 30 days. In still another group of rats, learning, was discontinued when with the first rat a record of 13 perfect trials out of 15 were obtained. This last arrangement was made to determine the stability of interaction of body parts under conditions of incomplete learning. A few rats of the remaining 21 of the second group were given 50 additional trials after the first perfect record was made. The rats selected in this last group showed that the extensor thrusts fluctuated either repeatedly, or very seldom.

After the circular maze was learned and a rest period of 30 to 75 days was given, no marked difference in redintegration was observed with the shorter or the longer period of rest. When 50 additional learning trials were given after the first perfect record, fluctuations continued, and no improvement in the redintegration series appeared. Very often a development of interaction of body parts for learning the maze cannot be facilitated when training is continued for many trials. The degree of development of interaction is as predetermined for learning as for relearning in the redintegration series.

Developed interaction of body parts for learning the maze problem, however, is more permanent when the reflex thrusts remain constant throughout the latter part of learning, the period of rest, and the redintegration series. Such a condition can be seen on table 49, which is the redintegration series from the learning table 28. A rest period of 30 days was given to the rat whose record is presented on table 49. From the seventeenth trial to the end of learning, all the reflex thrusts of this rat's limbs with the exception of the hind thrusts could no longer be evoked, and this condition of the thrusts prevailed throughout the redintegration series. A symmetry of a previous unequal antagonistic action of the neural centers of the rat's limbs has been facilitated by the disappearance of the extensor

TABLE 49

Redintegration series of learning table; rest period 30 days

NO.	TURNS					REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5	Fore	45 degrees	Hind	
1	P	P	P	P	P	None	None	Weak	Hypernormal
2	P	P	P	P	P	None	None	Weak	Hypernormal
3	P	P	P	P	P	None	None	Weak	Hypernormal
4	P	P	P	P	P	None	None	Weak	Hypernormal
5	P	P	P	P	P	None	None	Weak	Hypernormal
6	P	P	P	P	P	None	None	Weak	Hypernormal
7	P	P	P	P	P	None	None	Weak	Hypernormal
8	P	P	P	P	P	None	None	Weak	Hypernormal
9	P	P	P	P	P	None	None	Weak	Hypernormal
10	P	P	P	P	P	None	None	None	Hypernormal
11	P	P	P	P	P	None	None	Weak	Hypernormal
12	P	P	P	P	P	None	None	Weak	Hypernormal
13	P	P	P	P	P	None	None	Weak	Hypernormal
14	P	P	P	P	P	None	None	Weak	Hypernormal
15	P	P	P	P	P	None	None	Weak	Hypernormal

thrusts. Reflex excitability was always hypernormal. With the facilitation of symmetry of the neural centers of the spinal cord, fluctuations come to an end and in learning and in redintegration consecutive direct progressions to the goal persist. The redintegration trials are perfect. The constancy or the stability of reflex mechanism leads to the constant performance of movements. Repeated training is not so necessary.

TABLE 50

Redintegration series of learning table 25; rest period 75 days

NO.	TURNS						REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5		Fore	45 degrees	Hind	
1	–	P	P	P	P	1st	Both	Right	Weak	Hyponormal
2	P	P	P	P	P	1st	Both	R. wk.	Weak	Hypernormal
3	–	P	P	P	P	1st	Right	None	Weak	Hypernormal
4	P	P	P	P	P	1st	Right	None	Weak	Hypernormal
5	P	P	P	P	P	1st	Right	None	Weak	Hypernormal
6	P	P	P	P	P		R. wk.	R. wk.	Weak	Hypernormal
7	P	P	P	P	P		Both	None	Weak	Hypernormal
8	P	P	P	P	P		Left	None	None	Hypernormal
9	P	P	P	P	P		Left	None	None	Hypernormal
10	P	P	P	P	P		L. wk.	None	None	Hypernormal
11	P	P	P	P	P		Left	None	None	Hypernormal
12	P	P	P	P	P		Both	Right	Weak	Hypernormal
13	P	P	P	P	P		L. wk.	None	Weak	Hypernormal
14	P	P	P	P	P		L. wk.	Both wk.	Weak	Hypernormal
16	P	P	P	P	P		L. wk.	None	Weak	Hypernormal
17	P	P	P	P	P		L. wk.	None	None	Hypernormal
18	P	P	P	P	P		L. wk.	None	None	Hypernormal

On another table it will be seen that, when the extensor thrusts fluctuate, imperfect responses occur at turns though these responses did not occur for many trials at the end of learning. Imperfect responses may likewise occur when the fore extensor thrusts have disappeared and fluctuations occur in the neural centers for the production of these thrusts. In such cases, a symmetry of the centers is not facilitated. Table 50 is the redintegration table of the learning table 25. With the rat whose record is presented on these two tables, 50 additional trials were given after the first perfect trial. There is no great change in the reflex thrusts after a rest period of 75 days, and fluctuations in these thrusts are fairly constant. Either the right or the left ipsilateral fore thrusts, or both are evoked when the head is bent to one side of the midline of the body. The right or both ipsilateral fore thrusts are evoked when extending the head 45 degrees. The hind thrusts are weak or absent when the tail is dorsally extended. More active fluctuations in these thrusts occur in the early part of learning and in the beginning of the redintegration series. One thing must be noted, the left ipsilateral fore thrust predominates in the early part of learning, and in later trials the right ipsilateral fore thrust predominates. This fluctuation or reversion of predominance of the fore thrusts has produced, as previously noted, a change in progression through a different group of alternately place turns. At the end of learning, the predominance of the right ipsilateral fore thrust becomes effective. In the redintegration trials this predominance persists. The right ipsilateral thrust was present for a few of the early trials when the left was evoked. A reversion in the fore ipsilateral thrusts once more took place. On the first two trials both fore thrusts were evoked with the same extensor tone. Imperfect responses occurred in the first alley on the first and the third trials. Fluctuations in the extensor thrusts with the existence once more of hyponormal reflex excitability conditioned these imperfect responses. After the third trial, fluctuations in the extensor thrusts though frequent, did not condition the production of imperfect responses; retracing of steps in the alleys persisted until the sixth trial. After this trial perfect records were made for 12 trials. Evidently redintegration can once more be seen to be determined by the stability of the reflex thrusts and the maintenance of reflex excitability. An additional 50 trials produced no improvement in redintegration. Relearning was much more rapid than learning.

In another instance, when 50 trials were given after the first perfect record was made, there was produced less direct effect upon the efficiency of the responses of the rat, and this inefficiency will be seen to be due to active fluctuations in the neural centers of the cord for the production of the thrusts. This is

TABLE 51

Redintegration of learning table 30; rest period 75 days

NO.	TURNS						REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5		Fore	45 degrees	Hind	
1	—	D	—	D	—		None	None	None	Hyponormal
2	—	—	—	—	—		None	None	None	Hyponormal
3	P	P	—	—	P		None	None	None	Hyponormal
4	P	P	P	P	P		None	None	None	Hypernormal
5	P	P	P	P	P		None	None	Weak	Hypernormal
6	P	P	—	P	P		None	Mone	Weak	Hypernormal
7	P	P	—	P	P		None	None	None	Hypernormal
8	P	P	—	P	P		None	None	Weak	Hypernormal
9	D	P	—	P	P		None	None	None	Hypernormal
10	P	P	—	P	P		Left	None	None	Hypernormal
11	P	P	P	P	P		None	None	None	Hypernormal
12	—	P	—	P	P		None	None	None	Hypernormal
13	P	D	—	P	P		None	None	None	Hypernormal
14	P	P	P	P	P		None	None	None	Hypernormal
15	P	P	P	P	P		None	None	None	Hypernormal
16	P	P	D	P	P		None	None	None	Hypernormal
17	P	P	D	P	P	1st	None	None	None	Hypernormal
18	P	P	—	P	P		None	None	None	Hypernormal
19	P	P	P	P	P		None	None	None	Hypernormal
20	P	P	P	P	P		None	None	None	Hypernormal
21	P	P	—	P	P	1st, 3d	None	None	None	Hypernormal
22	P	P	P	P	P		None	None	None	Hypernormal
23	P	P	P	P	P		None	None	None	Hypernormal
24	P	P	P	P	P		None	None	None	Hypernormal
25	P	P	P	P	P		None	None	None	Hypernormal
26	P	P	P	P	P	1st	None	None	None	Hypernormal
27	P	P	P	P	P		None	None	None	Hypernormal
28	P	P	P	P	P		None	None	None	Hypernormal
29	P	—	—	—	P	2d, 3d	None	None	None	Hypernormal
30	P	P	P	P	P		None	None	None	Hypernormal
31	P	P	P	P	P		None	None	None	Hypernormal
32	P	P	—	P	P		None	None	None	Hypernormal
33	P	P	P	P	P		None	None	None	Hypernormal
34	P	P	—	P	P		None	None	None	Hypernormal
35	P	P	P	P	P		None	None	None	Hypernormal
36	P	P	P	P	P		None	None	None	Hypernormal
37	P	P	P	P	P		None	None	None	Hypernormal
38	P	P	P	P	P		None	None	None	Hypernormal
39	P	P	P	P	P		None	None	None	Hypernormal
40	—	P	P	P	P		None	None	None	Hypernormal

shown on table 51, which is the redintegration trials of the learning table 30. Again a rest period of 75 days was given, and throughout learning and the redintegration series of trials, the reflex thrusts can be seen on these tables to have been poorly developed. Often no extensor thrusts could be evoked. Reflex excitability in the early trials of learning and during the redintegration series was hyponormal. After 85 trials of learning, 16 perfect trials were obtained out of 18. The usual norm of 12 consecutive perfect records out of 15 could not be obtained. Before the sixty-ninth trial few consecutive perfect records were obtained. The redintegration series of trials, after 75 days rest period, revealed no marked changes in the reflex thrusts from the last of learning, but in the first 3 trials reflex excitability was hypornormal. Many imperfect responses occurred and these continued throughout relearning. After 40 trials, many consecutive perfect records could not be obtained. Retracing of steps in alleys leading to difficult turns occurred. Relearning was as difficult as learning. An additional 50 trials had no perceptable effect upon the development of interaction of body parts for progression through consecutive turns of the circular maze. Table 52 is the redintegration series of learning table, table 26. Fluctuations in the reflex thrusts were as great throughout redintegration as throughout the learning series. Reflex excitability was, in the first two trials of learning, hyponormal, but became hypernormal on the third trial and remained so to the end of learning, for 24 trials. Before the first perfect record was made, active fluctuations in the reflex thrusts occurred which produced difficult facilitation of progression through turns in the maze. In the redintegration series, active fluctuations in the reflex thrusts and in reflex excitability produced many imperfect responses at turns and in alleys. On this table both mechanisms, that of antagonistic action of the rat's limbs and that for the production of reflex excitability conditioned poor redintegration.

The tables presented of redintegration are typical of those of the maze problem. Many additional features in redintegration can be shown, for the unequal antagonistic action of the rats' limbs in extension and flexion varies greatly. Great variations in the records result from marked fluctuations. Sometimes after the period of rest of 30 or 75 days, perfect redintegration was obtainable, but if fluctuations were marked during this

TABLE 52

Redintegration of learning table 26; rest period 30 days

NO.	TURNS						REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5		Fore	45 degrees	Hind	
1	–	–	–	–	–	1st	Both	Right	Strong	Hyponormal
2	D	–	–	D	D	1st, 3d	Both	Right	Strong	Hyponormal
3	–	P	P	P	P		R. str.	R. wk.	Weak	Hyponormal
4	–	P	–	P	P		Both	R. wk.	Weak	Timid
5	–	P	–	P	P	1st, 3d	Both	R. wk.	Weak	Hyponormal
6	–	P	–	–	P	1st	Both	R. wk.	Weak	Hyponormal
7	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
8	P	P	P	P	P	1st	Both	R. wk.	Weak	Hypernormal
9	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
10	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
11	P	P	P	P	P		Right	R. wk.	Weak	Hypernormal
12	–	P	P	P	P		R. str.	Both	Weak	Hypernormal
13	P	P	P	P	P		R. str.	None	Weak	Hypernormal
14	–	P	–	–	–		Both	R. wk.	Strong	Hyponormal
15	P	P	P	P	P		Both	None	Strong	Hypernormal
16	P	P	P	P	P		Both	None	Strong	Hypernormal
17	D	P	D	P	P		Both	None	Strong	Hypernormal
18	P	P	P	P	P		Both	R. wk.	Strong	Hypernormal
19	P	P	P	P	P		Both	None	Weak	Hypernormal
20	P	P	P	P	P		Both	None	Weak	Hypernormal
21	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
22	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
23	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
24	P	P	P	P	P		Both	R. wk.	Weak	Hypernormal
25	P	P	P	P	P		Both	None	Weak	Hypernormal
27	P	P	P	P	P		Both	None	Weak	Hypernormal

period, redintegration was not alone imperfect, but also relearning was difficult or impossible, whereas learning was comparatively easy. The reverse may occur, marked fluctuations in the reflex thrusts may condition better redintegration. The antagonistic action of the rat's limbs may become more suitable for progression in the maze. Perfect records have been obtained after a 75 days rest period. In general, movements appeared slower in rats after 75 days of rest than after 30 days. The reflex extensor thrusts remained constant, and only a slight difference in excitability was noted in rats when a longer period of rest was given. This difference can not be significant, for other rats with as great a rest period as 75 days showed reflex excitability equal to rats given a rest period of 30 days. After a period of 75 days no perceptable retrogressive changes as is implied by a loss in retention have been noted to take place.

Since no perceptible effect on redintegration was noted in rats when 50 additional trials were given after the first perfect record was produced, it is of interest to note the effect on redintegration with incomplete learning. In two groups of rats of 7 each, learning was continued until in the first rat, 13 out of 15 perfect records were made. A rest period of 30 days was given.

Brockbank has shown that when learning is completed in one rat before it is facilitated in others, the redintegration tests do not show that the rapidity of learning is any indication of a possible perfect redintegration. Other rats' records in which learning was incomplete are usually better than that of the first rat in which learning was first completed. The results obtained by additional experimentation substantiates this fact. The conditions which determine better redintegration in one rat than in another after a similar period of training, may be due to marked changes in the extensor thrusts when reflex excitability remains hypernormal, to fluctuations in reflex excitability, or to fluctuation in the mechanism for both manifestations. Table 53 gives the learning test and the redintegration test of a rat in which learning was completed first, and table 54 similar records for a rat in which learning was incomplete when learning was complete in the first rat. In the first trial of the learning series of both of these rats, reflex excitability was hyponormal and was repeatedly so throughout learning. On the redintegration tests in table 53, reflex excitability was hyponormal and appeared at times throughout relearning. The other table 54, shows that the reflex excitability was hyponormal on the first trial and appeared at times throughout relearning. The first of these

TABLE 53

NUMBER	TURNS						TIME	REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5			Fore	45 degrees	Hind	
1	—	—	—	—	—		1890	Right	None	Strong	Hyponormal
2	—	—	—	D	—		145	Right	None	Strong	Hypernormal
3	—	—	D	D	—		25	Right	None	Strong	Hypernormal
4	D	P	—	—	P		7	Right	None	Strong	Hypernormal
5	P	P	P	P	P	1st	10	Right	None	Strong	Hypernormal
6	P	P	P	P	P		7	Right	None	Strong	Hypernormal
7	—	P	P	P	P		8	Right	R. wk.	Strong	Hypernormal
8	—	P	P	P	P		8	Right	R. wk.	Strong	Hypernormal
9	—	P	P	P	P		7	Right	R. wk.	Strong	Hypernormal
10	D	P	P	D	—		12	Right	None	Weak	Hyponormal
11	—	—	—	P	P		15	Right	None	Weak	Hyponormal
12	P	P	P	—	D		14	Right	R. wk.	None	Hyponormal
13	D	P	P	P	P		9	Right	None	None	Hypernormal
14	P	P	P	—	P		7	Right	None	Weak	Hypernormal
15	P	—	—	—	P		30	None	None	None	Hyponormal
16	P	P	P	—	P		12	R. wk.	None	Weak	Hyponormal
17	—	P	P	—	P		7	R. wk.	None	Weak	Hypernormal
18	P	P	P	P	P		7	R. wk.	None	Weak	Hypernormal
19	P	P	P	P	P		6	None	None	Weak	Hypernormal
20	P	P	P	P	P		9	None	None	Weak	Hypernormal
21	P	P	P	P	P		12	Right	None	Weak	Hypernormal
22	P	P	P	P	P		9	Right	None	Weak	Hypernormal
23	P	P	P	P	P		6	None	None	Weak	Hypernormal
24	P	P	P	P	P		6	None	None	Weak	Hypernormal
25	P	P	P	P	P		6	None	None	Weak	Hypernormal
26	P	P	P	P	P		7	R. wk.	None	Weak	Hypernormal
27	P	P	P	P	P		6	R. wk.	None	Weak	Hypernormal
28	P	P	P	P	P		6	R. wk.	None	Weak	Hypernormal
29	P	P	P	P	P		6	None	None	None	Hypernormal
30	P	P	P	P	P		6	None	None	None	Hypernormal
31	P	P	P	P	P		7	R. wk.	None	Right	Hypernormal
32	P	P	P	P	P		6	R. wk.	None	Right	Hypernormal
Redintegration 30 days											
1	—	—	—	—	—		90	R. wk.	None	Weak	Hyponormal
2	—	P	—	P	D		23	R. wk.	None	Weak	Hypernormal
3	D	P	P	P	P		17	None	None	Weak	Hypernormal
4	—	—	—	P	P		29	R. wk.	None	Weak	Hyponormal
5	P	P	P	P	P		7	None	None	Weak	Hypernormal
6	P	P	P	P	P		6	None	None	Weak	Hypernormal
7	P	P	P	P	P		6	None	None	Weak	Hypernormal
8	P	P	P	P	P		6	None	None	Weak	Hypernormal
9	P	P	P	P	P		5	None	None	Weak	Hypernormal
10	P	P	P	P	P		6	None	None	Weak	Hypernormal
11	P	P	P	P	P		5	None	None	Weak	Hypernormal
12	P	P	P	P	P		6	None	None	Weak	Hypernormal
13	—	P	—	P	P		12	None	None	Weak	Hypernormal
14	P	P	P	P	P		10	None	None	Weak	Hypernormal
15	P	P	P	P	P		21	None	None	Weak	Hyponormal

TABLE 54

NUMBER	TURNS						TIME	REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5			Fore	45 degrees	Hind	
1	–	–	–	–	–		370	Left	None	Strong	Hyponormal
2	–	–	–	–	–		248	None	None	Strong	Hypernormal
3	–	D	–	–	–		56	Left	None	Strong	Hypernormal
4	–	P	–	P	P		26	Left	None	Strong	Hypernormal
5	–	P	–	P	D		55	Left	None	Strong	Hypernormal
6	–	P	–	P	P		14	Left	None	Strong	Hypernormal
7	–	D	–	–	–	3d	16	None	None	Weak	Hypernormal
8	P	P	P	P	P		13	None	None	Weak	Hypernormal
9	P	P	P	P	P	1st	11	None	None	Weak	Hypernormal
10	–	P	P	P	P		12	Both	None	Strong	Hypernormal
11	P	P	P	P	P		13	Left	Left	Strong	Hypernormal
12	P	P	P	P	P		7	Left	Left	Strong	Hypernormal
13	–	–	–	–	P		51	Left	Right	Strong	Hyponormal
14	P	P	P	P	P		8	Left	Right	Weak	Hypernormal
15	P	–	–	–	–		20	L. wk.	Right	Weak	Hyponormal
16	P	–	P	–	P		15	L. wk.	Right	Weak	Hyponormal
17	P	P	P	P	P		7	L. wk.	L. wk.	Weak	Hypernormal
18	P	P	P	P	P		6	L. wk.	L. wk.	Strong	Hypernormal
19	P	P	P	P	P		15	L. wk.	Right	Strong	Timid
20	P	P	P	P	P		8	L. wk.	None	None	Hypernormal
21	P	–	–	–	P	1st	28	L. wk.	Both	Weak	Hyponormal
22	P	P	P	P	P		14	L. wk.	Right	Weak	Hyponormal
23	P	P	P	P	P		9	L. wk.	None	Strong	Hypernormal
24	P	P	P	P	P		8	L. wk.	Left	Weak	Hypernormal
25	P	P	P	P	P		14	L. wk.	Right	Weak	Hyponormal
26	P	–	P	P	P		12	None	None	Weak	Hyponormal
27	P	P	P	P	P		12	Left	None	Weak	Hyponormal
28	P	–	P	–	P	3d	19	None	None	Strong	Hyponormal
29	D	–	P	P	P		13	None	Right	Strong	Hyponormal
30	P	D	P	P	D		19	None	None	Strong	Hyponormal
31	P	–	P	–	–		13	None	None	Weak	Hyponormal
32	D	–	–	D	–		20	None	Both wk.	Weak	Hyponormal
Redintegration 30 days											
1	–	P	P	P	P		16	Left	Both	Strong	Hyponormal
2	P	P	P	P	P		8	Left	Both wk.	Weak	Hypernormal
3	P	P	P	P	P		6	Both	Both	Strong	Hypernormal
4	P	P	P	P	P		7	Right	Right	Strong	Hypernormal
5	P	P	P	P	P		6	Both	Both	Strong	Hypernormal
6	P	P	P	P	P		6	Left	Both wk.	Strong	Hypernormal
7	P	P	P	P	P		6	Left	None	Strong	Hypernormal
8	P	P	P	P	P		7	Left	None	Strong	Hypernormal
9	P	P	P	P	P		6	Left	Left	Strong	Hypernormal
10	P	P	P	P	P		6	Left	Left	Strong	Hypernormal
11	P	P	P	P	P		6	Left	Left	Strong	Hypernormal
12	P	P	P	P	P		7	None	None	Strong	Hypernormal
13	P	P	P	P	P		6	None	None	Strong	Hypernormal
14	P	P	P	P	P		6	Both	Right	Strong	Hypernormal
15	P	P	P	P	P		6	None	Both wk.	Strong	Hypernormal

tables shows that at the end of learning 15 perfect records were obtained, whereas the second table shows a very imperfect and poor record when the same number of trials were given. Absence of fluctuations in reflex excitability have produced better redintegration series on table 53. In addition in this table it will be noted that at the end of redintegration a more marked fluctuation in the extensor thrust occurred without production of imperfect responses indicating a bettering of interaction after a period of rest.

The results obtained from these last two tables confirm those obtained throughout this investigation on redintegration; the redintegration trials as well as learning are more effective when an unequal antagonistic action of extension and flexion of the limbs is least in evidence and when fluctuations in this action does not take place; that is, movements to learn the problem in every trial are more effectively performed when fluctuation in certain mechanistic parts are not of frequent occurrence. This effectiveness of responses is often dependent upon a manifestation of hypernormal reflex excitability and should excitability fluctuate, then movements are again ineffectively performed. Redintegration is dependent upon the stability of important reflex mechanistic parts for learning. This stability permits an adequate development of interaction of body parts for learning. It is scarcely necessary to imply that there has not taken place in an organism a fixation of definite or of successful movements to the exclusion of all other movements. With the removal of the idea of the fixation of a "successful act," there is no need for the assumption that there has taken place a development of paths in the nervous system or a reintegration in this system for the exclusive performance of a "successful act."

The movements performed in previous trials are not "recalled," or do not reoccur in the succeeding trials because definite "sensory impressions" are made in the organism. There does not usually take place the production of a definite movement with the reappearance of an appropriate stimulus, or are the "successful" movements again produced because of the reappearance of a definite stimulus or stimuli. With each succeeding trial, an interaction of body parts is developed still more, so that a coördination of integrated movements can be performed to solve a problem. The configuration of the problem produces coördinated movements for which this interaction has been developed and not otherwise. The organism is after each trial differently constituted, or it is functionally different. The rapidity with which it becomes different is dependent upon the inherent development of individual parts of the organism. Repeated fluctuations in the reflex parts limit the development of interaction much more so than when the reflex parts are undeveloped and remain constant. The effect these fluctuations have on such a development of interaction is clearly seen in this investigation of redintegration.

When redintegration is poor, it is not due to a "loss in memory," in "retention," in "sensory motor responses," in "sensory experience," or in any similar "psychological process." For it is evident no retrogressive changes in neural structures can be said to occur with which these processes are supposed to be associated. In redintegration, no retrogressive changes appear to take place, but a destructive effect is produced on developed interaction of reflex mechanistic parts brought about by active fluctuations of these mechanisms. The problem of redintegration is purely a physiological one.

The question may naturally arise how does this coördination persist after a period of rest, even from trial to trial. Some are inclined to believe that there is an underlying mystery to this problem. The movements used in solving problems are fundamental integrated movements that are conjoined or coördinated. The neck reflex and the extensor thrusts are inherently conjoined with progression and with no other fundamental movement as the scratch reflex or biting reflex. These movements persist because they are fundamental movements already in a measure coördinated and when used in an exaggerated form must interact more pronouncedly with other body parts. This interaction is developed for the exaggerated performance of movements as any physiological process is in connection with active secretion of a gland, or when in active muscular exercise the efficiency and contraction of skeletal muscle, or the contractions of the heart, increases when interacting with other body parts. The mechanical efficiency in this case resulting from greater interaction of body parts persists in the same manner as the efficiency of the coördinate movements during learning persists. In both instances this efficiency is more lasting if the activity of the parts is continued, and a rest period of no great extent is instituted. This fact for learning has been clearly demonstrated in the investigations of redintegration. Again fluctuations in any reflex mechanism disrupts developed interaction for the performance of fundamental movements as the usual decrease in efficiency of an organ of the body disrupts the working of other organs of the body.

IX. THE LEARNING OF SEVERAL PROBLEMS IN SUCCESSION

The direct physiological effect of learning a problem has been shown to be the development of an interaction of mechanistic parts of an organism, and the actual effect on developed interaction for learning one problem when several are given in succession is from many points of view of interest. There arises the important biological question of the adjustment of an organism to different environments and its consequence. Of no less interest are questions of the advantage of "previous experience," "transfer," "interference" in learning and the question of learning, and the age of the organism. Each of these questions have led in the past to special problems for investigations. From the standpoint of a physiological explanation of learning they are of renewed interest. The explanation of each of these problems may at the outset be considered in the light of the results already obtained in the present investigation of learning.

Without considering the physiological changes that take place in learning, and the significance these changes have in the rigid determination of all movements, any explanation of learning based on "sensory experience" is considering an aspect of learning not actual facts. That an organism "profits by experience" after one problem is learned when another problem is given, implies that an additional factor directs, controls, and aids in many ways the mechanisms in their application to the learning of another problem. The denial of the existence of such a factor would not be of advantage in the present investigation, but it can be said that the consideration of such a factor is unessential in the present investigation.

Experiments reported on the question of "transfer" in learning have considered to a great extent either the "sensory consequence" that similar or disimilar stimuli are likely to produce in different problems, or have held to the idea that with the existence of stimuli, responses in an animal must invariably be produced. The idea of the necessity of the production of a response when stimuli are present has led to an antithetical division of responses into negative and positive kinds, or into negative and positive "transfer effects." Recently Webb (53), Wylie (54) and Wiltbank (55) have undertaken an investigation of transfer of responses in the white rat with an idea that stimuli, or the environment common to several problems can reveal actual transfer effects. But it is evident that such an idea is not convincing. Assertions are made that eventually different stimuli may produce a response, and accordingly, there exists "generalized habits" which can be elicited by different stimuli. "Special habits" must then be supposed to be produced by less general stimuli. So frequently in animal psychology, analysis takes the place of incomplete observations. The physiological changes that take place in learning are of less consequence than analysis of stimuli and their possible consequence on an organism.

Again, much of this analysis of the tabulated results obtained in the investigation of learning in animals has been concerned with the idea that learning eventually leads to some definite end product, a habit. A great many subordinate views have to be added because of the existence of this idea. One view exists without any positive evidence, that one "habit" may "interfere" with the establishment of another, or that with the establishment of an additional "habit" a real conflict of the old and the new one being formed exists. In physiology we do not speak of one process conflicting or interfering with another, and why it should in behavior or in learning is difficult to understand. The idea of interference in responses or "habits" is directly opposed to that of a "transfer" of responses. The presence of unusual stimuli and the production of new movements "interferes" with the performance of the "successful" act already fixated, and thus interrupts the analysis of an established "habit."

Once more the apparent greater rapidity with which a "habit" is established in younger than in older animals has led to the belief that this rapidity in learning in younger animals holds true under all conditions. This idea exists often with the one that "transfer of habits" of some sort exists, or the experienced individual is of more value than the inexperienced. Two views of this sort retardation of learning in old age, and the value of experienced individuals, often conflict when an analysis of "habit formation" is not critically reviewed. Analysis of habit formation in terms of stimuli and response is the ruling passion of animal psychology and of behaviorism. The functioning of the entire organism is subordinated to analysis of "habit formation."

TABLE 55

NUMBER	NUMBER OF RATS	AGE	
		days	
1	20	40	Circular maze, rope ladder, sawdust-box, square maze (fig. 8) inclined plane
2	12	125	Circular maze, rope ladder, sawdust-box, square maze (fig. 8) inclined plane
3	12	60	Latch-box unsolved, square maze (fig. 9), rope ladder, sawdust-box, circular maze
4	12	60	Latch-box unsolved, square maze (fig. 8) rope ladder, sawdust box, circular maze
5	6	53	Latch-box, rope ladder, circular maze
6	15	40	Circular maze, square maze (fig. 8)
7	10	40	10 trials on rope ladder, circular maze, sawdust-box, and learned inclined plane

To consider such views in the present section of this investigation of learning would lead to no better results than have been attained in the past. An analysis of responses can scarcely lead to an understanding of the physiological changes that produce responses and facilitate them for learning. The changes that take place in the organism as a result of learning are of greater importance than the application of terms to certain aspects of learning. In the investigation of learning of several problems it is quite necessary to observe the effect produced on the rat as each problem is learned in succession. Observations of conflicting responses, "inhibition," "transfer" of responses, and the like, will conceivably not enlarge our view of the significance of learning a number of problems.

The problems used were the latch-box, the inclined-plane, the sawdust box, the circular maze, the square mazes (figs. 8 and 9) and the rope ladder. All these problems, with the exception of the sawdust box, have been used in the present investigation of the integrated movements in learning in the white rat. To observe to best advantage the changes in the development of interaction of body parts when learning several problems in succession, different arrangements of the problems are necessary. The order of presentation of problems, the number used for a given arrangement, and the ages of the rats are given in table 55. Of the 20 rats used in the first arrangement of problems, 5 were given a rest period of 30 days before a continuation of an investigation of learning the square maze, figure 8. This rest-period was instituted in order to observe any change that would take place in the organism after a period of idleness. With the exception of the last three arrangements of problems, a rest period of 3 days for preliminary feeding in problems intervened between the learning of each problem in succession. The 12 rats used when the square mazes were the first problems used were given these problems when after 6 days of 30 minute trials the latch-box remained unsolved. It was of interest to note the effect on the development of the interaction of body parts in learning after one problem remained unsolved, and later after a series of problems were learned, to note the general improvement in learning. The first four arrangements were most satisfactory, particularly the introduction of two problems, the rope ladder and the sawdust box, before giving the second maze. The object of this arrangement was to see if developed interaction for progression through alleys and through turns of the first maze persisted for the second maze after the intervention of other problems, and to what extent. In the fifth arrangement only two problems were used, the difficult latch-box problem and the circular maze. The object of this arrangement of problems was to compare the results obtained from learning the circular maze in the fifth arrangement. The results obtained from the sixth arrangement of problems, where only two mazes were used, can be compared with the results obtained from the preceding two groupings, particularly the last. In the seventh arrangement, rats were given 10 trials on each of the several problems. In such a grouping it is interesting to note the rapidity of the development of interaction of body parts as successive problems are given and whether, when a so-called habit is not established, there is any difference in partially learning successive problems, and completely learning successive problems. All these arrangements are it appears sufficiently complete to justify the existence or not of those aspects of learning which have been discussed above.

The ages of the rats were 40, 60, and 125 days. Preliminary investigation revealed the fact that rats differing in ages from 40 to 125 days generally showed a difference in the rapidity of responses made and in the continuation of movements. This conclusion must be regarded as a general one, for in some of the older rats rapid and continued movements exceeded those in many of the rats 40 days old. Rats differing in ages of 40 and 60 days revealed only individual differences, and no general statement can be made. The difference in ages of different groups of rats 40, 60, and 125 days old was deemed sufficient in order to observe any change in the development of interaction in learning the first and succeeding problems. If age has anything to do with retarding learning, a retardation in older rats ought to increase with every additional problem learned. The taking of rats at different ages from the confinement of the living cage, as Hubbart (56) has done and giving them a problem to learn is scarcely convincing that older rats learn with greater difficulty than younger rats. In the open, learning in any animal undoubtedly continues to old age and death, and the question arises whether when learning has been continuous, it is more difficult for an older animal to learn than for a younger one. When observing rats in the living cage, it has been noted that generally rats 60 or 125 days old which have been confined for some time, are less active than similar rats 60 days old that have learned a problem and are in the living cage only during a rest period of 30 days. None of the rats used may be regarded as old rats.

The rats for this investigation were from many different litters and only the strongest of them were selected. The condition of the reflex mechanisms, the reflex extensor thrusts, and that for the production of reflex excitability was not determined for all rats. The early part of the work was to observe the condition of rats and the performance of movements made. These things were taken into consideration with the rapidity of learning. Later the exact conditions which determined the results were sought.

The results obtained from the first four arrangements of problems are presented on tables 56, 57, 58 and 59. These tables give the number of trials required to learn each problem. With the exception of the inclined-plane problem, 12 perfect records out of 15 were required to be made. Because of the difficulty with which a number of perfect records were obtained when learning the inclined-plane problem, a criterion of 6 consecutive perfect records was demanded. In one instance no perfect records were made. In the sawdust box, directive integration to one spot to dig for 10 consecutive trials was the criterion accepted for this problem. There existed in this problem three different positions, front and two sides, in which digging could take place to enter the box. With all these problems, the effective performance of a definite series of movements was regarded as a perfect record.

In the first four arrangements of problems, the maze was the first problem given. Learning the maze problem has been

TABLE 56

Rats 40 days old

NUMBER OF RATS	CIRCULAR MAZE	ROPE LADDER	SAWDUST BOX	SQUARE MAZE I	INCLINED PLANE
1	40	41	20	21	26
2	29	60	20	27*	23
3	44	41	47	26*	21
4	29	46	19	29	29
5	52	52	30	22*	26
6	60	79	21	18	
7	31	98	30	19*	33
8	36	63	17	24*	41
9	30	56	32	27*	16
10	27	73	19	28	43
11	49	72	21	19	47

* Rest period of 30 days.

TABLE 57

Rats 125 days old

NUMBER OF RATS	CIRCULAR MAZE	ROPE LADDER	SAWDUST BOX	SQUARE MAZE I	INCLINED PLANE
1	38	45	21	29	58
2	40	54	36	21	42
3	41	50	32	21	34
4	42	68	18	27	91
5	48	37	32	29	36
6	48	32	29	36	24
7	35	50	31	19	26
8	44	67	28	32	20
9	32	45	31	36	67
10	43	52	27	69	55

TABLE 58

Rats 60 days old

NUMBER OF RATS	SQUARE MAZE I	ROPE LADDER	SAWDUST BOX	CIRCULAR MAZE
1	27	55	23	22
2	48	89	32	19
3	30	55	37	24
4	28	52	43	26
5	44	38	34	38
6	35	62	43	20
7	36	60	41	19
8	80	63	19	28
9	44	96	24	26
10	30	106	47	23

shown to result in the development of interaction of body parts to facilitate progression through consecutive alleys and turns. The problem is the same irrespective of the age of rats. Imperfections in progression are present only when the extensor thrusts are undeveloped. Since learning does not remove imperfections, the age of the rats apparently cannot remove or increase these imperfections any more than the manner of progression can be changed. If any difference exists in a rat's behavior when 40, 60 or 125 days old, it exists in the early trials during the facilitation of progression through consecutive alleys and turns of the maze. In rats 60 and 125 days old, retardation of the facilitation of progression is seen in the slower movements generally produced, particularly in rats 125 days old. Individual differences, however, have been noted in rats, 125 days old, when progression was as rapid as in any rat 40 days old. The slow movements generally observed appeared during progression through turns. At times, there appeared to be much less flexibility of the entire body in older rats. The attainment of posture for progression through turns was in some of these rats difficult to facilitate.

TABLE 59

Rats 60 days old

NUMBER OF RATS	SQUARE MAZE II	ROPE LADDER	SAWDUST BOX	CIRCULAR MAZE
1	46	65	21	46
2	65	40	51	40
3	51	52	35	21
4	44	81	20	23
5	50	68	48	23
6	34	54	28	25
7	62	28	30	38
8	45	35	70	28
9	45	42	55	30

In older rats after several trials, progression is facilitated with an increase in speed as it is with younger rats, yet often progression direct to the goal was made with less speed than in younger rats. At the end of learning, nothing definite could be said on this matter, for irrespective of the ages of rats, slight fluctuations in the degree of reflex excitability occurred in all rats.

Because of the slower movements produced in older rats and a retardation of the facilitation of progression in the early trials in the maze, a few more trials generally were required before the problem was learned than in younger rats. This can be seen in tables 56 and 57. A few more trials were generally required in rats 125 days old. There exists, however, the same range of individual difference in the number of trials to learn the circular maze when a difference in the number of trials is due to the functional condition of individual rats as was the case when rats learned previous mazes. A different functional capacity exists in rats for the facilitation of progression in the circular maze; that is, the development of interaction of many body parts for progression is not the same in all rats.

In succeeding problems, whatever the ages of rats, the rapidity of the development of interaction of parts for each problem determines the number of trials required to learn a problem. For the development of interaction for learning one problem than for another, the degree of the development of the reflex extensor thrusts of the rat's limbs is more adequate in some rats than in others. Conditions then are known which determine why one problem is easier to learn than another. This fact is evident from the results obtained from all the investigations of learning of previous problems. An increase in the number of trials to learn one problem and a decrease in the number for another problem takes place irrespective of the age of rats. With the addition of other problems, each problem does not become uniformly more difficult or easier as the age of each rat increases. Again, the increase in the age of a rat does not change the individual variation in the number of trials that were required to learn the first problem,

The persistence of individual variations in the number of trials required to learn a problem, irrespective of the age of the animal or the number of problems given, and the ease or the difficulty with which different problems are learned, does not indicate that a real "transfer" in learning takes place, but emphasizes more strongly the persistence of the functional condition of an organism when expressed in the degree of development of interaction of part of an organism. If anything is "transferred" or carried over from one problem to the next, it is the improvement of the general functional condition of the organism, and this is a physiological problem rather than one of a "transfer" of movements or of something to the next problem, or a question of identical stimuli which would produce certain movements. The general functional condition of the organism is improved upon after the first two problems are learned. In table 56 and table 57 it may be noted that fewer trials were required in rats 40 days and 125 days old in learning succeeding problems. This improvement is also noted in tables 58 and 59. The records presented in these last two tables were obtained from rats after the latch-box problem remained for 6 days unsolved.

Again, in rats of different ages, little difference if any exists in the number of trials required to learn the last three problems. When adding 3 days rest between problems to the number of daily trials required to learn each problem before the second maze was learned, it will be seen that rats that began the first problem at 125 days old were from 238 to 273 days old when given the maze problem; those of 40 days old were from 145 to 238 days old; and those that began with the square maze at 60 days old where from 159 to 238 days old when they were given the circular maze to learn. Many of the rats that were given the first problem at 40 days old and later were given a rest period of 30 days before the last maze was learned, there was exhibited an equal facility to learn as in other rats of the group that did not have this rest period. In addition, in the inferior group at 60 days old which were given the square maze as the first of the several problems, progression was facilitated at the ages from 159 to 238 days with fewer trials through the circular maze than it was through this last maze in those rats at 40 days old. When several problems are given, an increase in the age of rats has no effect upon the retardation of the development of interaction of body parts for progression in a second maze of a different type. At the time the last maze was learned, those rats that began the first maze at 125 days old were regarded as fully grown rats, and in such rats the last of a series of problems was learned as rapidly as in younger rats.

The statements made above hold true when the last problem was not a second maze, but another problem, the inclined-plane. An additional table, table 60, is presented giving the record of 20 rats 40 days old when the inclined-plane problem was given them to learn. When comparing the records of the inclined-plane problem in tables 56 and 57 with table 60, it is apparent that fewer trials were required to attain the criterion of 6 perfect records when previously 4 other problems had been learned than when this problem was the only one given. This is evident when in rats 125 days old a series of problems was begun, and these rats were 265 to 375 days old, when the inclined-plane problem was given. This problem was more easily learned than when this last problem was given alone to rats 40 days old.

TABLE 60

NUMBER OF RATS	TRIALS	NUMBER OF RATS	TRIALS
1	67	11	27
2	107	12	71
3	25	13	56
4	83	14	55
5	44	15	40
6	84	16	42
7	43	17	43
8	53	18	52
9	128	19	68
10	150	20	73

From the foregoing results, it is apparent that the functional condition of an organism greatly outweighs the importance of age as a factor in learning. Age is as secondary a factor as sex and temporary environmental influences. In a life less active than when learning, transitory environmental influences and other factors have been noted to effect an organism for a time, but the effect they produce on an organism is only temporary, and no evidence exists to show that learning is retarded, or is slower, because of such influences, or because of an increase in the age of an organism. When rats are taken at different ages from a living cage and given a problem to learn, age is not the factor which determines the slowness of learning in older rats, but the functional condition of the animals brought about by confinement in the living cage. The functional condition of the organism must be known before any generalization can be made of the effects produced by any of the things such as age, sex, or transitory environmental influences on an organism.

The behavior of rats shows that with every additional problem learned, a developed interaction of body parts aids the learning of succeeding problems. This developed interaction results in the ready attainment of posture for the production of specific movements. When an additional problem is learned, developed interaction of body parts for progression persists or is very soon facilitated in a new problem. Movements can then be produced in a definite direction, and if the problem box is a part of a problem, all movements are directed to the box. In the maze spurts and retracing of steps are greatly reduced in number. Though posture for progression is readily attained, it must be facilitated for the important movement that solves the problem box or for the different arrangements of turns in a maze. The two movements, direct progression and the important one which solves the problem box, were seen to be the most difficult to facilitate. It was exceedingly difficult in most rats to bring about posturing to raise the latch of the latch-box problem when it was shifted out of its usual position, and to respond to the ball and the cube when these were alternately placed in different positions. Usually a disruption took place in the coördinated integrated reflexes for posture for progression and in those for the attainment of posture to produce the important movement, and to a greater extent does this disruption occur when different problems are given than when shifting a problem from its usual position.

Much greater must be this disruption of coördinated reflexes for successive posturing when a great difference exists in the configuration of problem and much less so when a slightly different configuration exists in problems, as in the two mazes. Again, this posturing to perform the important movement is dependent upon the functional condition of the extensor thrusts in rats, and, accordingly, with the kind and degree of posture required, certain problems are more difficult for one rat than another. Different degrees of disruption would therefore occur in the inherent coördination of the two integrated movements, or dependent upon the development of the rat. The ineffective production of posture for the performance of an important movement as scratching sawdust, plunging the plane, or passing through a difficult turn may disrupt direct progression to the mechanism of the problem. This ineffectiveness was observed to be the case, when progression was continued around the latch-box problem, when ineffective posture before the latch took place, and when retracing of steps in alleys leading to difficult turns occurred in the maze. When several problems were given, direct progression was soon refacilitated. This refacilitation appeared very soon after the important movements of converging the limbs inward to cross the narrow path of the rope in the rope ladder problem, after the production of the scratch reflex in the sawdust box, or after the plunging of the plane by any of the usual methods were made. In the case of the two mazes, refacilitation of progression in alleys leading to easy turns occurred very soon, but was retarded through alleys leading to difficult turns.

To substantiate the statements made above, two additional arrangements of problems, the fifth and the sixth, were made. The fifth arrangement consists of quite different problems, the latch-box, the rope ladder and the circular maze; the sixth arrangement, the circular and the square maze (fig. 8). Observations of the movements of rats clearly revealed that with unlike problems, a greater disruption of the coördination of direct progression and the important movement occurred than when rather similar problems were given.

The results obtained from the two maze problems were practically the same as when two different problems intervened between the giving of the first and of the second maze to learn. Any variation in the behavior or the results obtained could be attributed to the functional condition of the reflex extensor thrusts that were recorded before each trial. A fluctuation in the reflex extensor thrusts occurred in some rats when the second maze was given, or at the time of preliminary feeding in the goal. A difference in behavior was noted in some rats with appearance of timidity, and not because of a change in the unequal antagonistic action of extension and flexion of the rat's limbs. In rats manifesting reflex excitability progression without the occurrence of spurts took place and retracing of steps in alleys between turns sooner disappeared. In some rats, no spurts or retracing of steps were made and progression was direct from turn to turn or in an alley beyond a turn. The reason for the appearance of spurts and the retracing of steps in the second or square maze was due to the closeness of progression to the wall of the square maze. This behavior showed that in the circular maze the attainment of posture for progression in alleys was facilitated in a circle and in some rats had to be facilitated for progression in a straight line in the square maze. A development of interaction for progression in the two alleys of the two mazes is slightly different, and for a short time with most rats, it produces a disruption of direct progression in the alleys.

This disruption of direct progression is, however, to be attributed more to the presence of turns than to the existence of straight alleys instead of curved alleys as in the circular maze. There appears much less retracing of steps through alleys, of the square maze than through the turns. Turns 1 and 3 in the square maze, (fig. 8) are right hand turns, whereas in the circular maze they are left hand turns. This difference in the turns increases the difficulty to facilitate progression not alone through the alleys, but also through the turns. Retracing of steps are frequent through turns on the first and second trials. On the third, or even the fourth trial, progression when direct in the alleys, continued beyond a turn or into a cul de sac. In the majority of the 15 rats used, consecutive direct progressions to the goal were facilitated from the fourth to sixth trial. In a few rats, direct progression through turns 1 and 3 and occasionally through 2 or 4 was not facilitated after the first 9 or 10 trials or at times throughout learning. Accordingly, disruptions of direct progression to the goal result more from a difficulty to facilitate direct progression through turns than through alleys. The interaction of body parts for the facilitation of posture for direct progression in the alleys was more persistent than when learning a second maze. The coördination of movements for direct progression and for the important movement of progression through turns is disrupted in a functionally developed rat only for a few trials, but disruptions persist longer when fluctuations in the antagonistic action of limbs occur.

When a greater difference in problems exists than is present in the two mazes, this disruption of the coördinate integrated reflexes for posture for direct progression and for the important movement is much more manifest, and particularly is this true if progression in a new problem is to be changed in direction or made more difficult. The disruption was very evident in the fifth arrangement of problems where the latch-box, the rope ladder, and the circular maze were used. The records of two rats are presented in tables 5 and 8 of the investigation of the latch-box, and tables 61, 62, 63 and 64 in this section of the investigation of learning of several problems. Tables 61 and 62 are the continuation of rat's record from table 5, and tables 63 and 64 are the continuation of the rat's record from table 8. The record presented on table 5 is approximately the average record obtained from rats when learning the latch-box problem, and table 8 one of the poorest records obtained from this problem. These series of tables were selected with a view to presenting some facts bearing upon the difficulty of learning one problem and not another, and in addition to emphasize the disruption of coördinate integrated reflexes in the attainment of posture for each problem.

The records of the two rats presented on tables 5 and 8, emphasize the difficulty with which learning in one rat in the latch-box is facilitated, and the comparative ease with which it is facilitated in another rat. Table 61 is the continuation of learning on the rope ladder of the rat whose record is in table 5. It will be noted that the facilitation of progression on the rope ladder is much easier than for a second rat whose record is given in table 63. The learning of the latch-box problem was in the second rat more difficult than in the first rat. Posturing in the latch-box problem is difficult and posturing for progression on the rope is again not easy to attain. The reverse was the case with the maze problem, facilitation of progression through con-

TABLE 61

Rope ladder

NUMBER		REFLEX THRUSTS			REFLEX EXCITABILITY
		Fore	45 degrees	Hind	
1	Put on rope	R. wk.	None	Strong	Hypernormal
2	Lost equilibrium	R. wk.	None	Strong	Hypernormal
3	Crossed	R. wk.	None	Strong	Hypernormal
4	Crossed	R. wk.	None	Strong	Hypernormal
5	Crossed	R. wk.	None	Strong	Hypernormal
6	Progression rhythmic	R. wk.	None	Strong	Hypernormal
7	Progression rhythmic	R. wk.	None	Strong	Hypernormal
8	Lost equilibrium	R. wk.	None	Strong	Hypernormal
9	Progression rhythmic	R. wk.	None	Strong	Hypernormal
10	Progression rhythmic	R. wk.	None	Strong	Hypernormal
11	Progression rhythmic	R. wk.	None	Strong	Hypernormal
12	Progression rhythmic	R. wk.	None	Strong	Hypernormal
13	Progression rhythmic	R. wk.	None	Strong	Hypernormal
14	Progression rhythmic	R. wk.	None	Strong	Hypernormal
15	Progression rhythmic	R. wk.	None	Strong	Hypernormal
16	Progression rhythmic	R. wk.	None	Strong	Hypernormal
17	Progression rhythmic	R. wk.	None	Strong	Hypernormal
18	Progression rhythmic	R. wk.	None	Strong	Hypernormal
19	Progression rhythmic	R. wk.	None	Strong	Hypernormal
20	Progression rhythmic	R. wk.	None	Strong	Hypernormal
21	Progression rhythmic	R. wk.	None	Strong	Hypernormal
22	Lost equilibrium	Right	None	Strong	Hypernormal
23	Progression rhythmic	R. wk.	None	Strong	Hypernormal
24	Progression rhythmic	R. wk.	None	Strong	Hypernormal
25	Progression rhythmic	R. wk.	None	Strong	Hypernormal
26	Progression rhythmic	R. wk.	None	Strong	Hypernormal

TABLE 62

NUMBER	TURNS						REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5		Fore	45 degrees	Hind	
1			Failed				R. wk.	None	Weak	Hyponormal
2			Failed				R. wk.	None	Weak	Hyponormal
3	–	–	–	–	–		R. wk.	None	Weak	Hyponormal
4	–	–	–	–	–		R. wk.	None	Weak	Hypernormal
5	–	–	–	–	–		R. wk.	None	Weak	Hypernormal
6	–	–	–	–	–		R. wk.	None	Weak	Hypernormal
7	–	–	D	D	D		R. wk.	None	Weak	Hypernormal
8	D	D	P	P	P		R. wk.	None	Weak	Hypernormal
9	–	P	P	P	P		R. wk.	None	Weak	Hypernormal
10	D	P	P	P	P		R. wk.	None	Weak	Hypernormal
11	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
12	P	P	–	P	P	1st	R. wk.	None	Weak	Hypernormal
13	P	P	–	P	P	1st	R. wk.	None	Weak	Hypernormal
14	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
15	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
16	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
17	–	P	P	P	P	1st	R. wk.	None	Weak	Hypernormal
18	–	P	–	P	P	1st	R. wk.	None	Weak	Hypernormal
19	–	P	–	P	P	1st	R. wk.	None	Weak	Hypernormal
20	–	P	P	P	P		R. wk.	None	Weak	Hypernormal
21	–	P	–	–	P		R. wk.	None	Weak	Hypernormal
22	–	P	–	–	P		R. wk.	None	Weak	Hypernormal
23	–	P	P	P	P		R. wk.	None	Weak	Hypernormal
24	–	P	P	P	P		R. wk.	None	Weak	Hypernormal
25	–	P	P	P	P		R. wk.	None	Weak	Hypernormal
26	–	P	P	P	P		R. wk.	None	Weak	Hypernormal
27	–	P	P	P	P	1st	R. wk.	None	Weak	Hypernormal
28	–	–	P	P	P	2d	R. wk.	None	Weak	Hypernormal
29	–	P	P	P	P	1st	R. wk.	None	Weak	Hypernormal
30	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
31	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
32	–	P	–	P	P		R. wk.	None	None	Hypernormal
33	P	P	P	P	P		R. wk.	None	None	Hypernormal
34	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
35	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
36	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
37	–	–	–	P	P		R. wk.	None	None	Hypernormal
38	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
39	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
40	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
41	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
42	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
43	D	P	P	P	P		R. wk.	None	None	Hypernormal
44	P	P	P	P	P	1st	R. wk.	None	None	Hypernormal
45	–	–	–	–	P		R. wk.	None	None	Hyponormal
46	–	–	–	P	P		R. wk.	None	None	Hyponormal
47	–	–	–	–	P		R. wk.	None	None	Hyponormal
48	P	P	–	P	P		R. wk.	None	None	Hypernormal
49	–	P	P	P	P	1st	R. wk.	None	None	Hypernormal
50	–	P	P	P	P		R. wk.	None	None	Hypernormal

secutive alleys and turns to the goal of the circular maze was more difficult for the first rat whose record is presented on table 62. For the same rats one problem is more difficult to learn than another.

TABLE 63

Rope ladder

NUMBER		REFLEX THRUSTS			REFLEX EXCITABILITY
		Fore	45 degrees	Hind	
1	Put on rope	None	None	Strong	Hypernormal
2	Crossed	None	None	Strong	Hypernormal
3	Crossed	None	None	Strong	Hypernormal
4	Lost equilibrium	None	None	Strong	Hypernormal
5	Lost equilibrium	None	None	Strong	Hypernormal
6	Crossed	None	None	Strong	Hypernormal
7	Crossed	None	None	Strong	Hypernormal
8	Crossed	None	None	Strong	Hypernormal
9	Lost equilibrium	None	None	Strong	Hypernormal
10	Progression arhythmic	None	None	Strong	Hypernormal
11	Progression rhythmic	None	None	Strong	Hypernormal
12	Progression rhythmic	None	None	Strong	Hypernormal
13	Progression rhythmic	None	None	Strong	Hypernormal
14	Lost equilibrium	None	None	Strong	Hypernormal
15	Progression arhythmic	None	None	Strong	Hypernormal
16	Lost equilibrium	None	None	Strong	Hypernormal
17	Lost equilibrium	None	None	Strong	Hypernormal
18	Slipped	None	None	Strong	Hypernormal
19	Progression rhythmic	None	None	Strong	Hypernormal
20	Progression rhythmic	None	None	Strong	Hypernormal
21	Progression rhythmic	None	None	Strong	Hypernormal
22	Progression rhythmic	None	None	Strong	Hypernormal
23	Progression rhythmic	None	None	Strong	Hypernormal
24	Progression arhythmic	None	None	Strong	Hypernormal
25	Progression rhythmic	None	None	Strong	Hypernormal
26	Progression rhythmic	None	None	Strong	Hypernormal
27	Progression rhythmic	None	None	Strong	Hypernormal
28	Progression rhythmic	None	None	Strong	Hypernormal
29	Progression rhythmic	None	None	Strong	Hypernormal
30	Progression rhythmic	None	None	Strong	Hypernormal
31	Progression rhythmic	None	None	Strong	Hypernormal
32	Lost equilibrium	R. wk.	None	Strong	Hypernormal

The conditions which determine this variation in the learning of different problems is from previous investigations of learning in other problems quite obvious. The functional condition of the rat's reflex thrusts as indicated in tables 5 and 61 is more suitable for the attainment of posture to raise the latch and for progression on the rope than as is shown in table 62 for progression through alleys and turns in the maze; the reverse is the case with the other rat's records, whose results are presented in tables 8, 63 and 64, the attainment of posture for learning the first two problems is difficult and much easier for progression through the maze. The reflex thrusts of the first rat remain strong from the last trials in learning the latch-box, throughout learning the rope ladder and the maze. The right ipsilateral fore thrust is alone evoked and progression, as is the rule, is

TABLE 64

NUMBER	TURNS						REFLEX THRUSTS			REFLEX EXCITABILITY
	1	2	3	4	5		Fore	45 degrees	Hind	
1	–	–	–	–	–		Both	None	Strong	Hypernormal
2	–	–	–	–	–		Both	None	Strong	Hypernormal
3	–	P	–	–	–		Both	None	Weak	Hypernormal
4	P	P	–	–	P		Both	None	Weak	Hypernormal
5	P	P	D	P	P		Both	R. wk.	Weak	Hypernormal
6	P	P	–	P	P		Both	None	Weak	Hypernormal
7	P	P	–	P	P		Both	Left	Weak	Hypernormal
8	P	P	P	P	P		Both	None	Weak	Hypernormal
9	D	P	–	P	P		R. wk.	None	Weak	Hypernormal
10	P	P	P	P	P		Both	None	Weak	Hypernormal
11	D	P	D	P	P		Both	None	Weak	Hypernormal
12	D	P	P	P	P		Both	None	Weak	Hypernormal
13	P	P	P	P	P		Both	None	Weak	Hypernormal
14	P	P	P	P	P		Both	None	Weak	Hypernormal
15	P	P	P	P	P	1st	R. wk.	None	Weak	Hypernormal
16	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
17	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
18	P	P	–	P	P		R. wk.	None	Weak	Hypernormal
19	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
20	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
21	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
22	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
23	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
24	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
25	P	P	P	P	P		R. wk.	None	Weak	Hypernormal
26	P	P	P	P	P		R. wk.	None	Weak	Hypernormal

difficult through turns 1 and 3 of the circular maze. The effect of the antagonistic left neural center on the right is at times manifest in the occurrence of imperfect responses at the fourth turn. In the second rat, the extensor thrusts of the limbs are constant through the last trials of the latch-box and the rope ladder, but fluctuate greatly before the last trial on the rope ladder and before the first one in the maze. This fluctuation in the reflex extensor thrusts makes the maze problem less difficult. It is apparent that it is the functional condition of a rat which determines why one problem is easier or more difficult to learn than another.

The most striking behavior in rats when an additional problem is given occurs in the early trials. There may be manifestations of timidity, an increase in reflex excitability with active movements performed, or a decrease in reflex excitability with a decrease in movements, often amounting to inaction. When inaction is present a problem may remain unsolved. Manifestations of hyponomal reflex excitability is recorded for the first three trials on table 62. No period of rest was given before the next problem was offered. When reflex excitability persists with the performance of active movements, as it does in many rats then responses are more directive. These observations on the first trials have given rise in the past to elemental analysis of one sort or another. It is evident, however, that variations in the functional condition of rats accounts for differences in the behavior of rats.

It is this difference of behavior which has given rise to such impressions that "transfer" may be "positive" or "negative," or an absence of "transfer" may be evidenced. These are relative terms and indicate the functional development of individual rats rather than "transfer effects." In one rat when "positive transfer effects" are observed, posture for direct progression is evident in the second problem, and excitations in the external senses can direct progression. The attainment of posture to produce the important movements that actually solve the problem must be facilitated for a greater coördination with that for direct progression. All this is evident when one problem seems easier to solve, and to be more difficult to learn than another. When progression is produced by the external senses and posture for progression does not persist with the giving of another problem, "negative transfer effects" are present, and when inaction or slow wandering progression appears, an "absence of transfer effects" is evident. In these last two instances, posture for direct progression and for the production of the important movements must be refacilitated. Evidently different applications of the term "transfer" indicate individual differences in rats to learn a problem. Any explanation for the existence of the term is quite inadequate, unless it is the supposition taken from analytical psychology that "transfer effects" are determined in another problem by the presence of similar stimuli, and negative "transfer effects" are determined by the absence of stimuli in one problem which existed in a preceding problem.

In the same analytical way as "transfer" of definite responses, a "generalized habit" is "transferred" from one problem to the other. The existence of such a "habit" as something definite is questionable, but a general improvement of many parts of the animal enables them to work together with greater facility with the addition of each problem. A developed interaction of body parts for greater coördination of integrated movements common to all problems is facilitated. There results a facilitation for posture and for direct progression, but there are great variations in its persistence. Since one problem is more difficult for one rat than another, disruption of direct progression may result in the appearance of wandering movements. An improvement, as a result of training, is less evident, but the early facilitation of direct progression takes place. A decrease in the number of trials to learn succeeding problems testifies to this early facilitation of direct progression.

In whatever way analysis had led to the idea of "transfer" in learning, it must also have led to the acceptance of the belief of "interference" of responses, of "habits," and the occurrence of "retroactive inhibition" in learning. Innumerable movements can be said to "interfere" with the performance of the right movement when a "negative transfer" or an "absence of transfer" is supposed to be present. When one "habit" "interferes" with the learning of another "habit" possibly conditions are then equivalent to "negative transfer" and a "detachment" of some kind from an "established habit" is necessary. It is evident that an analysis of behavior of this kind assumes that "interference" is general and all behavior is not the result of the individual functional condition of rats. Additional investigations with the sixth arrangement of problems where two mazes, the circular and the square maze, were used, indicated that so-called interferences in responses, in "habits," resulted from a difficulty to attain posture for progression through straight alleys, and through right hand turns instead of left hand turns. In some rats during progression through straight alleys of the square maze, after progression was facilitated through circular alleys, progression in short spurts close to the inner wall of a straight alley occurred. At other times, posture for progression could not be maintained in a straight alley, and progression continued back and forth in front of the entrance of the maze. With other rats, progression could be accomplished through straight alleys, but imperfect responses were produced at a turn. In fact imperfect responses were made at turns with all rats. These variations in behavior reveal individual differences in rats, and it is the functional condition of the rat that is significant, not an analysis of what a "rat does." The selection of cases where one movement "interferes," or one "habit" does not work smoothly with another when the rat "does something" will lead to endless analysis and not to fundamental conditions which determine learning.

Again, the term "retroactive inhibition" has been used when the disturbing effect of the learning of a second problem is recognized upon the retention of a previous problem. Once more such terms as "positive" or "negative retroactive inhibition" and an absence of this "inhibition" are used. Evidently such terms refer to individual differences, and the functional condition of different rats. From the great variation in the results obtained in the investigation of redintegration and from the conditions that determine these variations, the term "retroactive inhibition" may be classed with such terms as "interference" and "transfer" in learning. Using these terms is as good as saying we do not understand learning.

One additional arrangement of problems must be considered, the seventh. Here 10 trials were given on the rope-ladder, circular-maze, sawdust-box and then 30 trials were given on the inclined-plane problem. This arrangement was given to see, if after direct progression had been established, but not a complete facilitation of this movement with the important movement that solves the problems, as was the case with other arrangements of problems, direct progression would persist through succeeding problems and to what extent. It was also interesting to determine, whether when learning was complete and a so-called "habit" established, this "habit" aids the learning of another problem. Observations leading to a definite conclusion in respect to the first interest will determine that of the second one.

When 10 trials were given in each of the problems, direct progression persisted when one problem was given and not in another. This prevailed with all rats. The same results were obtained in the first three arrangements of problems, and no difference in the behavior of rats was noted when a rat was transferred from one problem to the other when 10 trials in each problem were given, or after the learning of each problem could be said to be complete. After 10 trials on each problem, 30 trials given on the inclined-plane problem did not improve the methods used to plunge the plane, but directive integration was soon facilitated and consecutive perfect records were sooner obtained than when previously this problem was given independently of other problems. The establishment of a "habit" or "habits" is not an "incentive" or even an aid to learning other "habits" as is often implied, but the functional condition of the organism is improved, because an interaction of many parts has been developed and coördinated integrated reflexes facilitated.

To give several problems in succession does not make all rats alike. Uniform training does not make all learning a problem with the same ease, but when several problems are learned, another problem is learned with greater ease. The same general variation in the number of trials required to learn a problem exists in the first as in the last problem. This is due to a difference in the functional development of rats, and an improvement in general in learning is the result of a better development of interaction of body parts. The variations in the number of trials required to learn the problem, and the fact that one problem is more difficult to learn than another does not indicate that an organism profits by "experience," unless it is intended to mean that improvement in learning another problem results from a better working together of many parts of an organism.

In the learning of several problems, one thing seems quite significant, every important movement made that solves a problem becomes better linked up with progression. The capacity with which such a movement can be better coördinated with extension and flexion of the limbs in progression is dependent upon the degree of development of the antagonistic action of the rat's limbs and the functional development of the mechanism of reflex excitability and other reflex parts. An undeveloped condition, or an unequal antagonistic action of the limbs can reduce the effectiveness of responses. Movements become less directive and particularly is this true when reflex excitability is hyponormal. This undeveloped condition existing in one mechanistic part determines why one problem is more difficult to learn than another. In biological terms it would signify that an organism is more readily adjusted to one environment than another. The degree of the development of interaction of body parts must be significant in the survival of an organism.

X. THE PHYSIOLOGY OF BEHAVIOR AND LEARNING IN RATS

The investigations of learning in the foregoing series of problems point unmistakably to the possibility of a physiological explanation of learning. When one considers such mechanisms as the sensory, the rhythmic succession of extension and flexion of the limbs, that for the production of reflex excitability and others, every movement made can be said to result from an interaction of these mechanisms. The significant part played by some of these mechanisms is evident in the results obtained, particularly those results obtained from rats in which there exists marked unequal antagonistic action of extension and flexion of the limbs and when reflex excitability is either hyponormal or hypernormal. The significance of the sensory, respiratory and circulatory mechanisms is not underestimated when other reflex mechanisms are emphasized, but these are not so evidently concerned in learning. These last mechanisms maintain action of the other parts more essential to learning. Sometimes it appears difficult to say which is more significant in learning, the sensory mechanism, or such mechanisms as respiration and circulation. Many parts of the rat, however, are concerned in the development of interaction so that integrated reflexes can be facilitated for learning.

It is not apparent that the undeveloped condition of the two important mechanistic parts of the rat, that of the reflex extension and flexion of the limbs and that for the production of reflex excitability is the result of any undeveloped condition of such mechanisms, as the sensory, respiratory, or the circulatory which might directly interfere with the working of these two important parts in learning. Some evidence points to an undeveloped functional condition of other important reflex mechanisms. In case of an unequal antagonistic action of the rat's limbs, there is no direct evidence that in the centers of the cord for progression that a variation in the number of neurons in these centers exists, or that the efferent connections are either undeveloped or do not reach an average to produce an equal antagonistic of the limbs, but there is some evidence that other centers of the nervous system affect these centers. Brown (*57*) and Weed (*58*) have shown in the investigations of the centers in the brain which influence decerebrate rigidity that the cerebellum as well as regions near the red nucleus when stimulated, increase or decrease the extensor tone of the limbs. Excitation of the cerebellum may temporarily decrease rigidity, and stimulation of definite regions will produce the same effect. Stimulation of the region of the left side of the red nucleus increases the tone of decerebrate rigidity in the right fore limb and decreases it in the left fore limb; the reverse is the case if the region near the right side of this nucleus is stimulated, increase appears in the left fore limb and a decrease in tone in the right fore limb. The tone of a contralateral hind limb may also be increased with an increase in a fore limb. Now stimulation of the regions near the red nucleus at times fails in some specimens to produce an increase in extensor tone in decerebrate rigidity in one or more limbs. Stimulation of the cerebellum does not, in some specimens, produce a decrease in extensor tone of the limbs. These results indicate possibly the higher centers of the nervous system are at times undeveloped, that the centers of the cord alone, or the centers in the cord and the higher centers are undeveloped. Recently I have observed in decerebrate cats the variation in increase extensor tone as the result of stimulation of regions near the red nucleus and a decrease in tone as a result of the stimulation of cerebellum, and I have also noted fluctuations in the extensor tone in the limbs after the first two or three stimulations of these regions of the brain.

When there exists an undeveloped condition of the reflex thrusts, posture and a succession of alternate movements of extension and flexion of the limbs during progression is impaired and the coördination of these movements with other reflex movements such as the neck reflex is ineffective. A functional undeveloped condition of the thrusts is the rule in rats, and exceptionally few rats show anything approaching an equal antagonistic action of them. Fluctuations in the thrusts are fairly constant, and during learning these fluctuations produce imperfect responses. These responses really result from a disturbance of developed interaction as a consequence of these fluctuations. In a similar manner fluctuations in reflex excitability produce the same effects. In general, learning, or the facilitation of a greater coördination of inherent integrated reflexes does not improve the functional condition of the thrusts, but there is a tendency with an increased facilitation, for them to disappear and to restore a symmetrical balance of centers in the cord. At times, it appears, coördination of integrated reflexes can not take place without a partial restoration of a symmetry of parts of the rat, for when fluctuations are frequent, and if the extensor thrusts do not tend to disappear, learning is impossible. Many imperfect responses are made.

A greater coördination of integrated reflexes can not take place without these reflexes interacting with other parts of the body. If reflex excitability is hyponormal, failure for a time to solve a problem results. Excitability must appear before solution can take place. When reflex excitability is hypernormal, solution of the problem takes place very soon and learning is rapid. When reflex excitability is at first hyponormal and then in a few trials becomes hypernormal, it is a question whether an actual development of the mechanism for its production takes place, or whether an interaction of body parts is for some reason not retarded. Possibly sensory excitations in general must increase the extensor tone of many reflexes before reflex excitability is manifest. But a separate mechanism for its production may form an essential component part. As soon as sensory excitations in general are produced and transmitted, relational changes chemical corrtelaes of different specific substances from the internal secretory glands become active, increasing the metabolic rate by controlling an increase blood supply to essential parts for the development of interaction of body parts. Whether the view of Cannon (59) that neural action or of Stewart and Rogoff (60) that an increase volume of blood to these internal glands increases the amount of internal secretions in the blood is not essential to the view that internal secretions are active in the development of interaction of body parts. In any case, specific sensory excitations are not exclusively necessary for the development of interaction in learning, nor are they exclusively necessary for the production of a specific response.

With an increase in the interaction of many body parts when reflex excitability is hypernormal, a stimulus from any part of a problem produces a response. Then it may be said that an external sense directs the organism and progression is, in that case, to definite parts of the problem. In the problems investigated, the stimulus of the latch produces the neck reflex to raise it; a stimulus from the plane produces the extensor thrust to plunge the plane; stimuli from the rope continue progression on the rope; and stimuli from the alleys and turns in the maze continue progression to the goal. These objects acting as stimuli set working together many parts of an organism for the production, we may say, of definite coördinate movements. Interaction of many parts as the result of the application of a stimulus produces solution of these problems. This interaction is evidenced in the integration of one neural center with another and the dependency of this integration on the interaction of many body parts for posture for progression, and for the performance of the important movement. The presence of reflex excitability to produce wide spread changes in the organism for the greater development of interaction is essential.

Of all the physiological changes that take place in learning, the extension phase of a reflex movement is more important than the flexion phase. The flexion of a limb is certainly not always necessary to produce an effective extension of a limb. Fluctuations in flexion of the limb produce changes as well as in extension, but changes in flexion of the limb were seen only in one case to produce pronounced effects in learning. The appearance of a strong flexor rebound produced constant imperfect responses in the maze problem. An extension of the limb is more significant in the performance of turning movements in the maze and for moving and pushing of objects about than flexion. The same thing may be said of other members of the rat's body. Extension of the neck is more potent in learning than flexion could ever be to move objects upward, and at the start of such an extension as the neck reflex movement, flexion of the neck is hardly necessary. An extensor thrust of some form is essential to plunge the plane, and its effectiveness and constancy are dependent upon its development. But the most delicate performance of the thrust is present when in the ball and the cube the feet are placed on either of these objects. Later in learning this problem, the substitution of vision shows how intricate and highly evolved is the thrusts of the fore limbs. The mechanical effectiveness of these thrusts is dependent upon the degree of their development, for this substitution occurs only when they are functionally developed, when fairly developed, olfaction is used, and when they are undeveloped, no substitution takes place. All this seems to indicate that an efficient integration of the centers for the production of the thrusts and the higher centers of the brain is dependent upon the function development of these centers. In such instances when fluctuations in the thrusts occur and this substitution is temporarily disrupted, or imperfect responses appear, ineffective integration of these centers is the rule. An adjustment to an environment when the higher external senses and neural centers would be involved would be impossible. A change in method or in the effectiveness of movements during learning is a direct consequence of the change in the integration of neural centers. All this is very evident after a period of rest and the beginning of the redintegration tests. In the higher animals a "ready modifiability of behavior" may be the direct result of fluctuations or rapid changes in neural centers. The significance of extension of the rat's limbs, particularly the fore limbs, and not flexion of them in learning seems to indicate as much.

When we consider the functional development of the reflex parts of an organism and their interaction with other body parts we approach a physiological explanation for learning. We have to consider learning which does not imply "intelligence," guidance of any sort, or control, and it is possible to speak of learning which points out the adequacy of the mechanisms an organism possesses. The "trial and error" concept of behavior and of learning with any antithetical division of movements, is a useless and a harmful concept for a scientific explanation. This is evident even when it is assumed, without investigation, that there exists a morphological basis for learning or behavior, with the denial that there is present the adaptiveness of intelligence. Possibly the weakest point in this concept of learning is how the retention of the "successful" or the "congruous" movement is brought about. A psychological explanation with the introduction of "pleasure" or "sensory intensity" cannot be associated with any sound physiological explanation, and far more implausible would a personal estimate of movements, when selected from a great many an organism produces, result exclusively in modifying the nervous system for the retention of these movements. In learning there appears to take place an exaggerated performance of a prepotent reflex, such as the neck reflex, in coördination with rhythmic progression. The coördination of these movements exists before training, and an exaggerated performance of them must be developed. This is accomplished by a greater interaction of body parts being developed. An improvement in the performance of the movements results and persists as is characteristic of any physiological process that repeatedly functions. Individual differences in "memory" or in "retention" are to be attributed to the functional condition of the organism, and a supposed loss in any of these "psychological processes," or in the degree of the persistence of traces of previous "experiences," trials, or "habit" does not take place, but results from the constancy or the fluctuations of reflex mechanisms.

In such an explanation of learning the existence of such things as "habits" and as "instincts" is questionable. It appears no physiological explanation has ever been given to support the existence of these antithetical concepts. These concepts may be useful in any analytical procedure, but the burden of proving a real difference existing between them rests upon the users of these terms. "Habits" are supposed to be what the organism can acquire and not what it can not acquire. As a result the entire conception of learning leading to the acquisition of "habits," is unsatisfactory. With the prevalent conception of "habit formation," the functional condition of the organism is to a great extent lost. Comparisons of results which lead and do not lead to learning and the adjustment of the organisms to an environment are lost. An adjustment to an environment and its limitations can not be understood in terms of the acquisition of something, a "habit," but can be understood when the functional condition of the organism is considered, and the capacity of this condition to develop an interaction of body parts, which will show a functional relation of the organism to an environment. This functional relation was observed to be present when in the investigation of learning in a number of problems a development of interaction of functional parts of the rat's body was possible or impossible. The degree of the development of interaction rested entirely upon the inherent developments of parts and their fluctuations. Through learning these parts do not develop, or become functionally more effective, but because of their fluctuations are a constant source of disruptions to interaction of parts for learning, or for an adjustment to the environment of the problem. The limitations of an organism are defined by the functional condition of the organism, not by the ability to establish a "habit" or a number of them.

The functional relation of the organism to its environment is accordingly better understood by regarding at all times the functional condition of the organism which makes no distinction between so-called "habits" and "instincts." There are, of course, problems in learning which are so complex that this functional relation to the environment is ill suited. This must be evident when, with a rat with a fully developed reflex mechanism, an inadequate adjustment to an environment takes place, or the facilitation of a functional relation of body parts to an environment is impossible. The mechanism of the organism then does not fit or is not suitable for that of the environment. There are limitations to any independent mechanism.

In the present investigation of learning, the fitness of the organism to solve a problem was seriously considered. Early in experimentation the question as to the fitness of the rats arose when in the latch-box problem the proportion of the solutions of this problem was seen to be small, and the methods used to plunge the plane of the inclined-plane problem, depended upon the functional condition of the rat's reflex mechanisms. The question of fitness was also considered when the mechanism of the latch and the plane had to be adjusted to produce respectively the most effective neck reflex, and the extensor thrust of the fore limbs. Then the question arose whether variations in the functional condition of the unequal antagonistic action of the rat's limbs revealed in the condition of the extensor thrusts, or the absence of reflex excitability, constituted undeveloped functional conditions of an organism or biological variations. At that time it seemed paradoxical to say that variations in the reflex extensor thrusts were biological variations, for according to the Lamarckian view of variations these extensor thrusts ought to improve with learning, and Darwinian variations should show a degree of stability during the life of a rat. If the variety of unequal antagonistic actions of the rat's limbs revealed in the extensor thrusts are biological variations then these variations, it must be admitted, fluctuate during the life time of a rat. No previous evidence is to be had of biological variations fluctuating in the manner that is customary for the extensor thrusts. This fact led me to speak of the extensor thrusts being undeveloped. These variations in the extensor thrusts are often so great and so numerous that it is possible the variety of undeveloped condition of the thrusts are real biological variations fluctuating in the rat's life time. Similar fluctuations in the extensor thrusts of other animals have been noted.

In addition the mechanism for the production of reflex excitability does not improve during learning, for fluctuations in the mechanism occur to the very end of learning and almost invariably when reflex excitability is hyponormal in the beginning of learning, it is again hyponormal after a period of rest and the first trial or trials of the redintegration series. The inherent mechanism for the production of reflex excitability is it appears only temporarily improved by training. Fluctuations in the mechanism of reflex excitability is most marked in rats that manifest hyponormal excitability on the first trial of learning.

If the extensor thrusts and the mechanism for production of reflex excitability constitute real biological variations, then these mechanism and their fluctuations affect the entire organism in its functional relation to the environment more readily than any other variation it may possess. There may exist in these variations a starting point for adequate investigations for experimental evolution. The effect of the extensor thrusts is revealed in the learning of problems, particularly when the right ipsilateral fore thrust predominates. This predominance of the right fore thrust or neural center over the left ipsilateral neural center, seems to indicate an evolutionary development in the direction of right handedness. But this variation, tending to right handedness, is in functional relation with other parts of the organism, and is revealed in the development of an interaction of body parts in specific situations: in the facilitation of direct progression to the plane of the inclined plane problem, and in direct progression to the goal of mazes when one direct path exists in mazes. In some problems the predominance of the left ipsilateral fore thrust persists. A complex functional relation of the rat to its environment is revealed in the predominance of the right ipsilateral fore thrust in some problems and the predominance of the left ipsilateral fore thrust in a few other problems. When the left ipsilateral fore thrust predominates, its extensor tone is unusually strong. This effect of the extensor thrusts in the rat's relation to its environment is again seen when these thrusts are functionally well developed, and an adjustment to the environment of the problems is readily facilitated.

Evidently if the environment of the problem remained for many generations the same, either selection would be among those rats in which the functional relation of the rats to the environment could be facilitated, or survival would be among those rats in which responses occurred that would be favorable to the existence of rats in a new environment. Survival would not take place because the establishment of a so-called "habit" for improvement of the extensor thrusts and other functional parts does not take place. When natural selection is supposed to be effective, survival is dependent upon selection acting upon elementary factors. For survival, selection would be effective when acting upon the important movements that solve a problem in the manner learning is supposed to be dependent upon the selection of the "successful" movement from "random" movements. But it would be inconceivable that natural selection would be effective when acting upon specific movements when the essential thing for the production of these movements is dependent upon the physiological interaction of many body parts. Still less conceivable is it that natural selection would act upon any specific part of the organism, for these parts must interact with other parts to produce the important movements. Natural selection would not be among individuals who possessed a definite variation, but would be where a physiological interaction of body parts could be developed for an adjustment to an environment. Survival would then depend not upon any particular variation in the extensor thrusts or in the mechanism for reflex excitability but upon the degree of development of interaction of body parts, as is the case with learning. If survival is dependent upon natural selection acting upon developed interaction of body parts, natural selection as well as the selection of the "successful" movements are needless terms, for an adjustment of an organism to an environment or a problem, can be explained by saying that survival and learning in an animal are the direct result of the inherent capacity of interaction of body parts, developing for the production of suitable responses. An adjustment to an environment and to a problem resides in the functional condition of the inherent parts of an organism. The condition determining survival also resides in the fluctuation of the reflex mechanisms of the organism, for under one condition of these mechanisms survival would be more certain than under another.

XI. CONCLUSIONS

Functionally, an organism must be looked upon as composed of mechanistic parts, the interaction of which produces a characteristic behavior. No one part acts entirely independently, and each part that acts is affected to a greater or less degree by others. When undeveloped parts are present, ineffective movements appear, and the limitations of these parts are revealed when in interaction with other body parts. The possibilities for the development of interaction are indicated in the investigation of learning, and in the observation of the behavior of rats. Accordingly, it is not found necessary to posit any independent factor as an aid in the explanation of learning or of the rat's general behavior.

Every movement is predetermined by the functional development of the organism. Consequently we have to consider the effectiveness of the parts of an organism in their functional relation to the environment. The functional relation of the organism to its environment may be conceived, and the respective importance of interacting parts of the organism for learning and for survival may be thus determined. A ready facilitation of interaction of parts of the organism is to be found in the functioning of important reflexes considered along with the degree of reflex excitability manifest.

More often in rats the functional condition of parts is not the best. Only when it is the best are movements effectively performed. When an unequal antagonistic action of extension and flexion of the rat's limbs is present, the behavior of the rat changes, and this is because interaction of body parts is not completely facilitated. Fluctuations in the reflex thrusts or in the unequal antagonistic action of the limbs temporarily disrupts interaction of body parts and movements are ineffectively performed. In addition when reflex excitability is hyponormal and when reflex thrusts are undeveloped, interaction is difficult to facilitate. Fluctuations in the mechanism of reflex excitability may also occur during learning, and excitability may become hyponormal. At such times ineffective responses may be very marked. Inaction for a time prevails, and not until fluctuations in the same mechanism again occur does excitability become hypernormal. Reflex excitability and interaction are not something in themselves, but are terms which express the functional condition of the organism for learning and for survival.

Learning consists of the facilitation of fundamental movements, not the creation or the establishment of new paths or reintegrations in the nervous system. Something new is not established in the organism, but the facilitation of the performance of fundamental movements in an exaggerated form for the solution of problems is brought about. Integrated fundamental movements are more intricately coördinated and these movements interact with other reflex parts of the rat's organization. The end result of learning is not the establishment of a habit, but a greater facilitation of parts of the rat's body.

Variations in the functional condition of an organism at any time in the rat's life cycle is a better explanation for behavior than a consideration of "habits," "instincts" and "intelligence." There is no necessity, because of the supposed presence of such things as "habits," "instincts," or "intelligence," to say that the rat "does something." Consideration of fundamental movements, and integrated reflexes, and fluctuations in them and other body mechanisms, furnishes a more exact physiological basis for the study of the behavior of animals.

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CONSCIOUS ANALYSIS VERSUS HABIT HIERARCHIES IN THE LEARNING PROCESS

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It is the two-fold purpose of this paper to submit certain criticisms of an assumed function of consciousness in learning, and to emphasize habit groups or hierarchies as an important explanation principle in the learning process. The criticisms are based directly upon experimental data. The theory of learning was suggested by the experimental evidence, but it is offered as an independent consideration. The implication is clearly made in many of our standard texts that the rôle of consciousness is analytical—that the learner first analyzes the learning situation and then proceeds to master it. It is against this interpretation that the criticisms are directed. As opposed to this or to any attempt to read consciousness into the learning process, stand a number of behavioristic theories. The one now presented stresses certain behavioristic principles.

We are thoroughly familiar with the point of view which ascribes to consciousness a function, particularly in the initial stages of the learning process. It works as a directing agent. The beginner at typewriting is asserted to initiate his attempts at the expense of an increased or heightened amount of conscious activity; later, as an expert typist, he is supposed to perform with a minimum of conscious direction. Our concern is possibly not so much with the truth of this general statement as it is with its implications and concrete applications.

The success with which the behaviorist has explored (and exploited) the human learning process has resulted in the side-tracking of this issue. The modern investigator has been busy with graphs and tables, with plateaus and steeples. He has either dismissed consciousness altogether from his scheme of things, or he has acquiesced unprotestingly with this theory, and then ignored it as a small matter. The conclusions advanced in this part of the paper, in themselves, argue neither for nor against behaviorism. But they are frankly directed against the belief that consciousness directs affairs in the beginning stages of learning and then relinquishes them as automaticity appears, on the grounds that this explanation, however plausible it may sound as a generalization, breaks down upon concrete application.

In order to discuss with some degree of definiteness this alleged rôle of consciousness, invariably stated in the broadest possible way, it might be well to raise a few preliminary questions. In the first place, it may or may not be true that conscious activities are unusually in evidence during the first periods of the learning process. The point is at least debatable. If consciousness is present, however, to an intensified degree, it may be emotional rather than ideational—a possibility not provided for in the accounts ordinarily given. And as such, it may inhibit rather than facilitate the learning process. But, assuming that this over-production of consciousness is in evidence, and that it is ideational, does it constitute a particularized consciousness, not observable in non-learning situations? While none have made this specific claim, many have implied as much in their statements. Again, we should like to know the exact nature of this initial learning consciousness. Does it function by selecting the correct reactions and eliminating the incorrect? And finally, we are interested in the exact conditions under which heightened conscious processes are stimulated. Are they aroused in connection with new, or with difficult, or with complex situations? We are informed that consciousness becomes heightened in times of difficult or inadequate adjustment; yet common observation would lead us to believe that it also attains its maximum degree of intensity during periods of hunger or of sexual gratification. One reads both the theoretical and experimental literature in vain for satisfactory answers to these questions.

The conclusions now offered are assuredly not final, but they do attempt to face these issues squarely. Our data would seem to show, in the first place, that the heightened initial consciousness of the human adult learner is a phenomenon which has been considerably over-stressed; and that it has been erroneously placed in the cognitive category, where it should have been described as essentially emotional. Furthermore, the evidence indicates that conscious analysis follows, rather than precedes, the act of learning; and that it by no means follows, even, the retention and selection of new and difficult coördinations with any degree of consistency. In short, reasons exist for believing that the human learning process, stripped of its accessory accompaniments, is necessarily an unconscious process.

These conclusions, as just formulated, of course demand explanation and evidence. As a matter of fact, they are not particularly radical. If they are apparently so, the impression may be due in part to difficulties of terminology. It may be urged, for instance, that "non-deliberate" should be substituted for "unconscious," in the above statement. Yet the question, as a whole, is something more than a verbal one. Nor can it be disposed of as a metaphysical issue. Our data are largely introspective, and in minimizing the importance of "ideas," therefore, we are likewise assigning less importance to the "neural correlates of ideas." But it should be acknowledged that the question is a relative one. Other writers have called attention to the fact of unconscious learning, and it remains to be seen if the emphasis placed upon that principle in this paper is justified.

The investigation now reported consisted of a series of tests of motor ability, given to a group of 100 adult subjects. Since a number of trials were made by the subjects in each test, it can properly be regarded as an investigation of the initial phase of the learning process. Some of the results were obtained from a group of five adults, who practiced at card sorting over a period of three months, meeting five times a week. Particular reference will be made in this discussion to three of the seventeen motor tests—the Bogardus fatigue test, a group of balancing tests, and a "coordination" test. The apparatus for the last named consisted essentially of a triangle and a quadrilateral, cut as grooves in an aluminum plate. The subject, with a stylus in his right hand and a corresponding one in his left, was required to describe these figures simultaneously, at a standard speed, regulated by a metronome. It was in substance a test which interfered with the habitual coördinations of the two arms. The responses elicited by these three tests seemed to be fairly representative of the human learning process. Both introspective testimony and systematic notes on learning behavior were recorded, and both were used as a basis for the views now submitted.

Two facts, clearly brought out by the introspections and to all appearances by the observations on behavior, are chiefly responsible for these views. It is certain that difficult motor coördinations may be acquired which more or less completely baffle all attempts at analysis on the part of those who acquire them. And it seems quite patent that coördinations may be selected and retained unconsciously, in the sense that the individual who selects and retains them does so with no memory of the first time the act was performed, and with no memory of a conscious plan to perform the act. That these two principles have something in common is suggested at once. They are, however, quite distinct, inasmuch as they were demonstrated under different objective conditions.

Introspecting upon the coördination test proved to be an exceedingly difficult task. Mastery of the test itself, for that matter, was by no means easy. And it should be recorded that the subject showed all symptoms of close attention to his learning problem. In fact, the first part of Angell's description of another learning process is equally applicable here: "While one is *learning* to write, consciousness is involved in the most alert and intense manner." But the analogy between the mental processes of one who learns to write, as given by Angell, and the mental activity of the learner at the coördination test is not so apparent. To quote the ensuing account: "The position of the hand, the mode of grasping the pen, the model to be copied, the movement necessary to form each part of the letter—all these things, one after the other, are the objects of vivid attention" (1). All of these items, even the movements involved, are fairly objective and concrete; and the learner, presumably a child in the grades, *aided by detailed and definite instruction*, undoubtedly attends after the manner described.

The difference between the two processes is due to the fact that no model of the essential aspect of the coördination test was given to the subject. The test calls for a definite series of simultaneous arm movements. Its difficulty may be ascribed to two facts: that these synchronous movements are neither symmetrical nor similar; and that they must be performed in a definite and varied sequence. Moreover, they must be executed, according to the directions, with free movements—an error was recorded each time the stylus came in contact with the metal plate. Now, when the subjects were asked to describe the objects of their attention, they mentioned the aluminum plate, the stylus, the metronome, the electric buzzer, which warned them of contacts with boundaries of the figures, and the fact that they were moving their arms; but they made no mention of anything resembling a conscious analysis of the coördination itself. Their attention, to summarize the testimony, was upon stimulus, and upon warnings of erroneous movements. It was not directed towards anticipatory ideas of the movements to be performed. To put it in another way, the subjects did not deliberately form an idea of a movement-to-be-performed, and then proceed to express that idea in action.

These introspections—assuming, for the present, their reliability—have a negative value. They argue against the hypothesis which would make the acquisition of a motor habit depend and follow upon a clear-cut anticipation of the act, or upon a copying process of any definite sort. They also suggest an alternative and a positive explanation, so far as this particular test is concerned. Since the figures were grooves, cut into the plate, the movements of the subjects were physically restricted and guided by these grooves. The situation resembles, then, the problem confronting the individual or animal in the maze; and Watson's explanation of frequency of response, with the probabilities favoring the correct reaction as the one most frequently performed, would seemingly apply to the coördination test (2).

According to our interpretation, this theory, subject to certain important modifications and additions, does explain the human learning process in the coördination test. It does not, however, furnish a satisfactory account of the discovery of the correct reaction. Some more specific factor evidently functions. Psychological theory would be happily simplified if we could establish this factor as cognitive in nature—if it should turn out to be, for instance, the mental image. It happens, however, that the subject who found the task impossible was an advanced student in psychology, best fitted by training and by intelligence to analyze the process. It happens, too, that no correlation obtains between ability to introspect upon the process and ability to attain distinction in the test itself. If, therefore, the mental images, kinaesthetic processes, and anticipatory ideas *reported* by a few subjects were the mental process which actually functioned in learning, we must admit that they rendered feeble and fairly ineffective service. The factor, or group of factors, which must account for individual differences, is not the conscious analysis made by the learner of the learning problem.

A second example of the difficulty attending the attempt to read conscious analysis into the learning process is to be found in either of the two balancing tests employed in the motor test series. The subject was required to walk on the edge of a board, secured edge-up to the floor. He was asked to walk forwards and backwards, and to make a complete turn on this narrow footing—to learn, in a crude way, the art of tight-wire walking. He was also required to balance himself on one foot, Mercury-like, on a pedestal constructed of a piece of 2 by 4 inch plank, 16 inches in length, with a rectangular board at one end, serving as a foot-rest. Since this pedestal was not attached to the floor, the subject was forced to maintain an almost perfect balance. Both tests were conducted by trials. Errors were recorded for the first, time for the second. Introspections were called for in both.

Simple as these tests are, they do not lend themselves to introspective dissection. The instructor may demonstrate them, and he may give a few valuable hints, such as those regarding visual fixation points and the position of the feet; but his responsibility must cease with these meagre aids. The selection of the effective reflexes must be left to the learner. But how they were selected and permanently retained none of the subjects—including the writer—could tell. Certainly, not by any direct conscious process. It is true that these tests, involving equilibrium, place more of a premium upon inherited coördinations than tests such as the Bogardus. But they were none the less difficult, and none the less subject to improvement; they were, therefore, true learning problems.

Moreover, they elicited intense and definite conscious reactions. But this is just the point of the discussion; the fact that a type of consciousness is aroused, in connection with an initial learning situation, which is clearly not engaged with the analysis of that situation, logically argues against the function usually ascribed to consciousness as a universal law. The nature of that consciousness will be discussed subsequently.

By way of contrast with coördination and the balancing tests, the Bogardus fatigue test affords an instance of a learning problem which permits, and to an extent demands, the fullest conscious approach possible. Its technique can be described verbally and demonstrated visually. The complete reaction elicited by it consists of a number of definite acts: the operator reaches for and grasps a small wooden cube with his right hand, places this cube on a marked square (a small platform, devised for the occasion, in this experiment), catches the cube with his left hand when it is knocked off by the revolving arms, etc. In this test we should expect the process of learning to write, referred to above, most closely to be duplicated.

In just this test, however, we find only an ambiguous support of this theory of learning. For, in the first place, some of these specific habits were acquired with a minimum, at least, of conscious direction; and in the second place, the habits which were consciously directed at the start continued to call for conscious supervision—prolonged practice failed to place them in the automatic class. The first point is advanced against the universality of the principle, even when overt acts are involved;

the second point is opposed to the assumption that the learning consciousness is a particularized or a unique consciousness.

One particular reaction made in the Bogardus test will serve to illustrate the principle of unconscious (at least, "non-deliberate") habit formation. In giving the preliminary directions to the subjects, no reference was made to the device of looking at the supply box, or tray, containing the wooden cubes. The discovery or employment of this was left entirely with the subject. It happens to be, however, a decidedly efficient method, and it was adopted by practically all of the subjects who made good records in the test. In a few instances, the anticipatory idea of looking at the tray was reported as preceding the act; in a greater number of cases, act and awareness of the act were simultaneous; but in as many other instances the acquisition of this act as a habit was accomplished without definite awareness. By this is meant that the subject had no memory of the first time the act was performed, and no memory of a pre-formed plan to adopt it.

Similar results were obtained from many other tests. In card sorting, the extremely useful device of employing the thumb and finger in the left hand, holding the deck, was acquired unconsciously by several of the subjects who were unaccustomed to card games. In the familiar ball tossing experiment, unconscious acquisitions of essential habits of manipulation were demonstrated by practically all of the subjects who practiced at this performance.

Acknowledgments should be made at this point of possible criticisms, directed against the introspections reported, and of the difficulty, in general, connected with the attempt to introspect under the conditions of the experiment. The subjects were, in the main, "untrained" in introspective technique. It has already been stated, however, that the few who did describe their conscious experiences failed, in so doing, to explain their learning process. They merely enumerated the sensory experiences which necessarily accompanied the movements involved in learning. The fact that no correspondence exists between ability to introspect and ability to learn is significant, since it indicates that the subjective processes capable of being observed and described were psychological by-products, of no real value in the learning itself. If this were not the case, it would follow that the professional psychologist should be able to distinguish himself in feats of skill. His ability, however, to assist himself or others in such accomplishments as dancing, swimming, or driving a car seems to be limited to his knowledge of general principles (fatigue, temporal distribution of effort, and the like) and to his analysis of specific efficient methods. But these are obviously empirical and objective affairs, and the psychologist, therefore, may be of service just to the extent that he is an efficiency expert.

One particular introspective difficulty, however, should be noted. It may be urged that our subjects, in the Bogardus test, were aware of the act of fixating on the tray, and that they merely lapsed into forgetfulness. But this explanation seems to be a bit gratuitous. It is based upon either an a priori theory that such attention is pre-supposed, or upon the facts that some of the subjects did report this definite awareness. The accumulating body of experimental evidence (Book, Swift, and others) corroborates the testimony of the subjects.

These instances have been cited, not for the purpose of maintaining the thesis that a state of inattention is the one best suited for the acquisition of motor habits, but with the end in view of emphasizing the reaction, rather than the idea of the reaction, as the most obvious feature of motor learning phenomena. The overt movements made by an individual to a motor test situation are largely independent of either his anticipatory ideas or of his full awareness of these movements. So far as the Bogardus test is concerned, it can only be said that definite and immediate awareness of the act described was conditioned by antecedent and rather incidental circumstances. It may be that a preliminary statement of instructions to the subject would facilitate his progress with the test; but in this event we would obviously have a different learning problem.

Although estimates of the quantity of total conscious processes are necessarily conjectural, the attempt was made to ascertain such fluctuations as could be observed by the subjects. The result of this attempt prompted the conclusion that learning consciousness is far from being unique, and that fluctuations in learning consciousness are primarily emotional. The initial decline, when it was in evidence, seemed due to the diminution of emotional activity, particularly as regards the state of embarrassment which was naturally aroused in the subjects at their first venture with the tests. The conclusion, however, is based primarily upon the testimony of three subjects who continued at card sorting during a period of six months. While they continued to exert themselves during this period, thus making it a period of continuous learning, they did reach a stage in which the actual movements involved became practically automatic. But the act of recognizing the cards remained a conscious act, and the total conscious reaction, including orientation, maintained a consistent level. If it is necessary to describe at all a *learning* consciousness, that description must be formulated in terms of effort, of conation in general.

The conclusions so far advanced have been negative in character, and possibly unjust in their disregard of certain obvious facts of daily experience. These facts will be provided for if a distinction is made between two fairly distinct types of human learning. In one of these, the problem is to establish a definite sequence of acts, no one of which, in itself, presents any unusual difficulty. Learning to drive a car is a case in point. A problem of this type lends itself readily to analysis in terms of a verbal formula, and it can be mastered by memorizing this formula. If verbal processes are essentially implicit motor habits, as Watson asserts, we have here the interesting phenomenon of one system of motor habits whose function it is to inaugurate another and a larger (explicit) system. It is this type of habit which may be acquired "consciously" and deliberately, in the sense that the learner memorizes and deliberately applies his formula. And, in due course of time, it may be dispensed with.

Closely related to this function of verbal processes is their employment for elaborating a plan of action. An excellent example of this—the reasoning process—in the field of motor adjustments is to be found in Watson's analysis of the individual called upon to face engine trouble (3). Again we find, even in this example, a minimum of conscious *directing* of thought processes. Angell discusses this point in his text, and goes so far as to state that: "It is easy for anyone who speaks readily to gain convincing evidence that the particular words used to express a thought were often in no sense mentally anticipated just prior to their actual use" (4). Whether the word and the thought are separate entities is not made clear in this passage. If so, the thought is in all probability an incipient act, a tabloid performance of the overt act, and as such, a reaction stimulated by the learning situation. The learner meets his situation, according to this interpretation, with a double repertoire, of reaction habits and language habits. Either or both may be elicited by the problem stimulus, and it is difficult to see how any directing agent could function in this double associative process.

That no such function is operative is seen in the second type of motor learning. This includes situations requiring skill, such as those which place premiums upon steadiness, accuracy, speed, the formation of minute and complex muscular responses, and the inhibiting of habitual and interfering responses. If the Bogardus test is an example of the first type, the coördination and balancing tests are instances of the second. Swimming might be mentioned as belonging to an intermediate type. Just to the extent that the responses called for permit verbal description, conscious analysis, in the sense indicated, is possible. But in the second type of problem—and this includes most of the tests used in the present investigation—acquisition necessarily precedes; and analysis, if made at all, follows.

All theories of the learning process agree in substance that it consists essentially of the selecting and retaining of adequate reactions, and of the rejecting and eliminating of inadequate reactions, from a surplus of movements. This principle is frequently stated in terms of habit, but without the implication that any given eliminated movement has necessarily reached a definite habit stage. And it is sometimes assumed that any isolated reaction in the series, irrespective of its ultimate fate, may be either a reflex or a habit previously formed. All of this, however, savors more of definition or description than of explanation. With this preliminary statement of fundamentals, the theories begin to diverge.

The considerations submitted in this paper have been to the effect that no anticipatory play of ideas uniformly precedes the selecting, retaining, rejecting, and eliminating activities. When such mental reactions do supervise learning, the process becomes either a reasoned problem, or one involving a step by step reliance upon a formula. After making full allowance for the rôle of verbal aids in learning, the essential aspect of the process remains unexplained; and it remains to be seen if any theory is at all satisfactory for this purpose.

One such theory holds that the successful act or group of acts usually terminates a learning trial, and that in the ensuing trial, it stands the best chance of selection because of its recency. If this explanation is to be taken on its face value, it cannot be of service so far as the present results are concerned. An arbitrary time unit distinguished the trials in the Bogardus and coördination tests, while task units (not achievement) separated the trials in the balancing, aiming, and card sorting tests. In fact, none of these tests was comparable in this respect to the maze or problem box experiment. The successful act was as likely to occur in the middle of the trial as at the end. And it should be noted, too, that in the learning attempts of practical life, the "trials" are not sharply defined.

In another type of explanation, the successful reaction is supposed to stand out in some way from the mass of unsuccessful attempts, other than by its temporal position. It is distinguished because of its feeling tone, or because of the accompanying increase in metabolism. According to a variation of this, it is marked by increased glandular or general organic activities in such a way as to insure its ultimate selection. Or, it is merely assumed to attract the attention of the learner, and in this way to take on a quality of vividness which will mark it as an object for conscious selection. In this group of theories, consciousness may or may not play a rôle.

In each of our tests, the learner was in a position, so far as objective test conditions are concerned, to note each success and each failure, and the second type of theory, therefore, merits consideration. One might conceivably admit the principle of non-anticipated discoveries, and yet claim that, once made, they were observed and consciously incorporated as habits. Against this solution, however, stands the fact that both the processes of acquisition and of fixation were at times unconscious; and the fact that, in the second type of learning problem, the learner was in a position to note results, not learning processes. The experimenter's observation, as well as the subject's reports, in the balancing tests, testify to the surprise of the subject, sometimes at his successes, sometimes at his failures. He was scarcely able to associate either with its antecedent causes. In the steadiness test, he was warned of each failure by the electric buzzer, but here again he was unable to describe his reactions immediately preceding the failure signal, in such a way as to differentiate it from his general reactions to the test. Even in the aiming test, in which he was apprised visually each time of his degree of success, it was obvious that no corresponding differences in kinaesthetic experience were observable. The same statement applies to the coördination test. In this, he was kept fully informed as to his progress, by visual, auditory, and kinaesthetic cues, but he was not made aware of causes. It is rather significant that, in a number of instances, subjects developed a decidedly fatalistic attitude towards their achievements: failures and triumphs were equally inexplicable. And in all cases, their explanations of specific results were either vague or general. It is clear, then, that if this theory implies a conscious association between a success and the movements directly responsible for the success, it does not explain the learning process; and if it does not presuppose conscious connections, it is all the more unsatisfactory.

No direct evidence is available which bears upon any glandular or organic theories. The difficulty here is plainly one of conception. To imagine that glandular changes parallel each attempt in the Bogardus problem, in which there were 43 per minute, is out of the question. And to introduce this theory as an explanation of gross results begs the question. More justifiable is the assumption that endocrinal activities may be useful in accounting for drive, perseverance, or motive.

In a third type of explanation, it is assumed that the adequate reaction is the one most frequently made. This certainly has the merit of simplicity. It is in harmony with current neurological beliefs, and with the more obvious facts of behavior. Furthermore, it dispenses with the somewhat artificial distinction made between acquisition and fixation. The question is whether or not either the objective conditions of the experiment or the nature of the reactions made favor the more frequent repetitions of the successful act. Watson has tried to demonstrate that the laws of chance so favor the animal in the maze, since it is forced, in any given trial, to follow the true path, but it is not under physical obligations to follow any other one sequence of paths (5). It is not our purpose to pass judgment upon this particular argument; but we may point out that emphasis must be placed upon the learning reaction itself, rather than upon the objective conditions of the task, if we are to employ this principle as a universal explanation. Clearly, the human learner is not physically coerced into placing the blocks correctly in the Bogardus test, in any possible sense.

That the nature of the learning process does distinctly favor the greater frequency of the successful act is made plausible when we introduce the principle of habit hierarchies. Possibly the maze furnishes only an ambiguous example of this principle, but if the theory possesses any merit at all, it must stand the test of examination in connection with a variety of learning situations. It has been rather distinctly assumed that maze habits are grouped in a coördinate series, with the path segment and its turn as the unit of this series. But an earlier investigation by the writer disclosed the fact that the human subject, at least, builds up first a general reaction and only subsequently a series of specific reactions. Incidentally, this fact was expressed in terms of ideas, but it can be stated just as fairly in terms of habit. The point was, however this may be, that the human adult finds himself equipped with an idea of the general direction of the true path, as the result of his first trial in the maze. Armed with this general idea, he is enabled to work out the details in ensuing trials—developing, of course, a successive series of ideas which become less general and more specific (6). If this does characterize the human learning process in the maze, we should expect something analagous to it to operate in the case of the animal, unless we are willing to assume a hiatus between human and animal forms of learning.

This may be regarded as a generalized habit, rather than an idea, as an illustration will show. It was ascertained in the same investigation that the human subjects, familiar with the location of the maze exit at the periphery of the maze, were thoroughly confused when they encountered a maze with its exit located in the center. Their reaction certainly was not one of weighing the possibilities, and then ruling against the central exit idea; rather, it was one of proceeding without ideational inhibitions towards the outside of the maze, after the manner of one who unthinkingly lights a loaded cigar. It was essentially a transfer of training phenomenon and it is probable that a rat so trained would make a similar error, as the result of similar interferences.

Something analogous to this successive and narrowing series of habit formations is to be found in each of the learning processes studied in our investigation, and this common principle is referred to in this paper as a habit hierarchy, suggested by the Bryan and Harter studies. In no instance was a complete learning achievement found to consist of a sequence of coördinate habit acquisitions. The first duty of the beginner at the Bogardus test was to acquire the gross arm movements; this mastered, his second task was to perfect the finer habits involved in picking up the cubes, placing them on the platform, catching them in his left hand. In the coördination test, he first learned to hold the tracers, to move them in their appropriate directions, and to do so with the metronome. He next discovered the trick of moving them simultaneously in the proper direction. The avoidance of contacts with the sides came early with some, later with others. This seemed to constitute a separate problem.[1] Probably the best example of this principle is to be observed in learning to dance—an accomplishment unfortunately not included in our experimental program. It is quite obvious in this learning problem that gross movements are practiced first, and that the finer ones, leading to ease, smoothness, and grace, are incorporated later. Balancing, too, affords an excellent example of this same phenomenon. The principle, as a descriptive fact, is not new: one at least similar to it has received considerable emphasis, for instance, from Judd (7).

There is no doubt of the tendency for the learner to proceed in temporal order from the coarser adjustments to the finer. That the habit zones were not sharply differentiated and that they over-lapped with wide margins scarcely need be mentioned. And it is quite possible that a learner might be forced under controlled conditions to acquire the minute habits first and the more general later. He might, for instance, practice at picking up cubes before reacting to the Bogardus apparatus in motion. In the case of the subjects already adepts at dealing cards, the essential finer coördination called for in card sorting was perfected before they were introduced to that test. But as a learning problem is ordinarily faced, in the laboratory and in life itself, the gross adjustments necessarily come first.

It will be noticed that the transition from the gross to the fine may show two different aspects. In some situations, different sets of muscles are brought into function. Arm movements, as opposed to hand and finger movements, would be a case in point. In other situations, the same muscles are seemingly called into play, but with an increased premium upon such kinaesthetic discriminations as are manifested in greater accuracy, rapidity, and steadiness. If the Bogardus test illustrates the first aspect, the balancing tests furnish an example of the second. In the first, the gross movements are frequently retained as permanent and necessary acquisitions; in the second, they are eliminated. In spite of the apparent contrast, these aspects possess a common feature, to be referred to subsequently.

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[1] An investigation of the motor learning processes of children is now under way in the laboratory. While the results have not been studied to date, it has been demonstrated that young children succeed with this test only by mastering each step at a time, beginning with the gross habits and gradually acquiring the finer ones.

The similarity between habit hierarchies as just defined and the generalized maze path habit is possibly not to be accepted without question. We can scarcely say that the rat shows gross movements for the outlines of the complete true path, and more refined movements for its individual segments or for *cul-de-sacs*. The animal evidently reacts in one essential way only, by running or walking. The similarity is more clear in the case of the human, occupied with the pencil maze. In that situation, direct introspective evidence justifies the distinction between the more extensive arm movements, and the movements restricted by maze unit segments. Since no discernible difference existed between the reactions of the human in the pencil maze and maze through which he walked, we may infer similarity as regards habit functions in the two. The point in common between the habit systems of the maze and those of the motor tests is to be found in the fact that the smaller habits are associated with the larger, and organized into the general reaction program. One may develop the habit of pacing back and forth across a room, as well as the habit of walking; and one may acquire habits of extending the arms over a piano key-board, in addition to the habits of key manipulation with the fingers. Of course, the act of crossing the room presupposes walking, but this difference is incidental.

The generalized habit, with its associated specific habits, finds additional illustration in daily experience. The first impulse of an individual familiar with a Dodge car, when he is called upon to drive a car with the standard gear shift, is to carry over the old habit into the new situation. The readjustment once made, he finds time to acquire the minor habits involved in grasping the new lever with the proper amount of force, etc. Or, we may say that the motor responses to a book are fairly uniform and general, but that the habits elicited by a prayer book and a dictionary differ. This, of course, is not to be taken in too literal a fashion: it is not implied that book, prayer book, and dictionary reactions comprise three distinct habits, or that the first is composed of the other two functioning conjointly. Habit hierarchies correspond to instinct groups; and psychologists no longer regard instincts as rigid entities, capable of formal classification.

If the learner responds to his problem by a narrowing series of habit adjustments, the conclusion is suggested that each step in the process not only prepares the way for, but actually arouses, the succeeding step. If we may express it in stronger terms, the learner is invited, at any stage of the process, to do the correct thing. At the risk of seemingly stressing the obvious, it may be pointed out that when he is seated before the Bogardus apparatus for the first time, he reacts to it in a manner generally correct. It does not elicit, for instance, swimming reactions. His initial movements are restricted by verbal directions and possibly by a demonstration, but they are also controlled by the visual stimuli, furnished by the apparatus itself. He is not literally led through the first movements in the way that the novice at dancing is lead through the steps, but he certainly is not required to choose a reaction from a series of suggested ones. As the result, then, of these combined stimuli, he learns to extend his arm to the supply box, where his fingers come in contact with the cubes. Here again, the most probable reaction is the one desired. The cubes are more likely to stimulate the grasping reaction than typewriting or piano reactions. The same statement may be made regarding his method of placing the cube, of catching it with his left hand, and of executing the other reactions called for. The consciousness involved is primarily perceptual, not in any sense unique or particularized.

In emphasizing this principle of the probably correct reaction, full allowance should be made for the fact that the first manner of meeting a situation calling for motor control, however spontaneous it may be, is frequently relatively inefficient, and at times apparently futile. The same, of course, holds for the first response at any stage of the learning. No one would claim that the efforts of one attempting to swim for the first time resemble the maneuvers of the expert swimmer. These efforts are indeed likely to result in drowning. Yet, within the scope of his repertoire of habits, the beginner at this problem comes more nearly doing the correct than the incorrect thing; and if mal-adjustments were not so immediately serious, he would in time acquire the correct coördinations. The heroic method observed a generation back, by which a boy was thrown into the water and was forced to work out his own salvation, may be criticized as inefficient, but it cannot be condemned as essentially and psychologically false.

According to this interpretation it is not necessary to assume that reactions are discovered by a trial-and-error process, and that consciousness then selects the one most fitting. Rather, the habit 'discovered' is the one brought by the stimuli, and the one most likely to be needed. If this is true, the chances are favorable for its re-arousal and permanent selection. But it is evident that on many occasions one of a number of coördinate habits may be stimulated, as when an option is presented in the maze. In this case, it may stand an even chance of being the incorrect habit. But we should remember that "incorrect" is a relative term. The raccoon who learned to master the problem box by standing on his head merely failed to discover the most efficient method. Many of our subjects developed inefficient reactions in card sorting, such as standing too far away from the boxes or holding the deck too high. So the false habit, unless specifically inhibited, becomes part of the final system.

The process of actual elimination, therefore, is shown only when an adjustment clearly proves itself to be a detriment to learning. It is possible that consciousness, while it neither necessarily discovers nor selects, may yet function by censoring habits which have crept into the reaction system. But again we find no logical necessity for this supposition. The phenomenon of elimination is clearly one of inhibition when it is not (as in Watson's example of the maze) a matter of unfavorable chances. Here we may distinguish between special inhibitions for specific reactions and a general inhibition due to failure of the gross reactions. For the first, we have the buzzer warnings of contacts in the coördination test, visual promptings of failures in the Bogardus and card sorting tests, pain stimuli where the animal receives an electric shock in the event of a false maze turn. As an example of the second type, of general inhibition, may be mentioned the individual who sinks at his first attempts at swimming, or the subject who steps off the board altogether in the balancing test. In the latter case, it is evident that the individual cannot react favorably at all to the problem situation. He is forced, therefore, to make a new attempt. If the new attempt is a reproduction of the old—and it frequently is—failure again results. The beginner must then either continue in his fruitless endeavors, or attack the problem with a different set of gross reactions. When special warnings of failures are given, the same readjusting process may be observed. The tendency, in this type of difficulty, is again for the learner to substitute a different higher-order adjustment. Thus, minor difficulties in the Bogardus test prompted the subject to change his position in the chair, and warnings of contacts in the coördination test led him to grasp his stylus in a different manner. The result of any inhibition, then, is apparently a change in the method of attack most closely associated with the difficulty. It is similar to the method of an individual who, unable to find an index card in a set, proceeds to go back a considerable distance and revise his general method of search.

With the human subject, the inhibition undoubtedly results in the "felt difficulty," and in an attempt, at times, to analyze this difficulty. This analysis is essentially an act of reasoning. But two important facts should be kept in mind. In the first place, the reasoning process may be unsuccessful, although the learner may finally make progress in spite of this fact. So far as the laboratory tests referred to in this article, including the maze test, are concerned, we are justified in saying that reasoning reactions were on the whole thoroughly unsuccessful. They did not offer a solution of maze difficulties, and they did not furnish effective ideational controls for balancing, aiming, and moving the arms correctly in the coördination test. The learners had no data upon which they could reason; they were merely aware of failures. It may be said, in this connection, that while reasoning is not necessarily exclusively a verbal process, it does function primarily in those situations which permit verbal description. If the interpretation of the observer may be trusted, the subject who experiences difficulty in these tests characteristically reacts with a series of fairly random verbal responses which express emotional states, volitional resolutions, and to a limited extent only, analytical processes.

In the second place, since idea and act are so closely associated, the view is again suggested that the idea itself is essentially motor in nature. It is certain that the same situation, or the same stimulus, which elicits the idea may also elicit the act. According to this view, the idea is either (1) the incipient act, or (2) the act translated into articulatory processes. In either case it is an implicit motor reaction habit, functioning as a stimulus for a more overt act. Whether or not the idea contains imaginal factors other than motor is a question not answered by our evidence. But the motor element seems to predominate in the thinking processes of the individual engaged with motor tests.

A detailed application of the principles which have been emphasized to each of our learning processes could not be made without additional experimentation. The systematic recording of all observable movements made by a group of subjects would necessarily constitute part of the desired data. But so far as our laboratory observations go, the principles do seem to hold. For purposes of rough observation, a group of five subjects were given a set of mechanical puzzles and directed to solve them. The first reaction to a puzzle was to grasp it and to pick it up. This led to a number of separate manipulations which included pressing, twisting, pulling, and bending the movable parts. A majority of these habits of manipulation were clearly aroused by the specific tactual and kinaesthetic stimuli furnished by the position of the hand and of the puzzle, and by the immediate antecedent habits. The inhibitions observed were in the main physical—the subjects ceased in their attempts to move parts which would not move. The correct responses were inevitable, rather than accidental, since these inhibitions tended to narrow the groups of responses. Again, in learning to walk and to balance the body on the edge of a board—possibly the most difficult learning process for any theory to explain—it was evident that the first efforts at equilibrium, involving extensive arm and body movements, were followed by more refined responses. A wild sweep of the arms would be replaced by a slight movement of the forearm, as inhibitions due to falling and to over-compensations developed. There was no evidence of a conscious plan to substitute finer movements for coarse, or to replace a long sweep of the arms by a more restricted one. The substitutions came "naturally." In these two examples, conscious directing, if present at all, was responsible for the gross movements involved, not the fine; but the gross movements were just the ones most easily performed, since they were the ones already established. Learning consciousness, in all cases, was correlated with drive, not mechanism.

The above considerations are offered, in part, because of the writer's conviction that an unfortunate contrast is too frequently made between habit acquisition and instinct. The one is regarded as something inexplicable, the other is disposed of in summary fashion as "innate." Yet in any concrete manifestation of either we may observe an organism bringing to bear upon its problem an organization of fairly crude reactions, out of which more refined adjustments are finally evolved. In either case, the most generally correct reaction is the one first aroused, and as the result of repetitions of the problem-stimuli, the final adjustment becomes elaborated. And in either case, intelligent and conscious analysis of the adjusting process is made possible by the development of the process itself.

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A DOG'S DIARY[1]

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I am a black-and-white cocker spaniel, and, now being three years old, I feel that my experience may be understood by others, and have decided to write some reminiscences, especially of my youth. The facts I give are entirely accurate, though some allowance must be made for my interpretation of them. I and my mistress, for instance, might have quite different ideas of certain episodes, and I will try to let her point of view be known sometimes, if it seems to be of any value.

I was born on the 28th of October, 1912, at the kennels of Dr. Stewart Cassard at Towson. In common with the other canines at the kennels, I have the surname of "Drassac"—a rather back-handed compliment to the owner of the Cassard kennels. I have a distinguished line of ancestors on both sides, but it seems to me that my guardians have hardly appreciated my lineage, for they have never had me registered, nor have I been on exhibition at any show. Therefore, the value of my points has not been considered professionally. Instead, I am taught all sorts of useless tricks with infinitesimal pieces of cracker as rewards. What are these in comparison with a blue ribbon, which I am sure I deserve! When I think of this, I sometimes become reflective, and humans think I am sullen, stupid or obstinate, but I am anticipating.

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[1] This account of a systematic attempt to teach a dog to associate words with objects was written in June, 1916, from the daily record which was kept by Miss Gilman during the progress of the work. Miss Gilman had considerable experience in the training of dogs under "home" conditions, and possessed the additional qualification of fondness for the animals. The work was carried on as a part of her work in the Department of Psychology, with all the advice and criticism possible. The results are especially important because the dog was proved capable of learning the tricks which are frequently exhibited as evidence for the learning of names of objects, and of performing them in a striking way: and yet there was not the slightest evidence obtainable that the animal could associate a word with an object. He responded to shadings of the experimenters' voice, which were so slight that it was sometimes difficult for those who were cognizant with the details of the performance to recognize them; and the uninitiated were easily convinced that he *did* "understand the words," although it was easy to demonstrate that such was not the case, by eliciting the same performances with entirely different words, but with the same inflection or shading. These results are in accordance with tests which have been made with other trained dogs, reputed to be able to associate words with objects Several such cases have been investigated but in none of them has such association been clearly demonstrated, although there is apparent association of words with actions. The form of the account was chosen without view to publication.

My life as a pup at Towson was very uneventful, but on January 17, 1913, I was brought in to 513 Park Avenue and told that I was to be trained by Miss Elisabeth Gilman, though I really belonged to the Psychological Department of the Johns Hopkins University. The psychologist came to look me over, and as he seemed satisfied, twenty-five dollars was sent to Dr. Cassard, and my pedigree was locked up in the archives of the University.

It is curious to remember how I looked then.[3] I was a fat little fellow about 10 inches long, with curly hair. Everybody petted me, and I surely needed sympathy, for it was lonely work to live in a house with humans instead of out-of-doors with my brothers and sisters and parents. I sometimes catch echoes of talk of hereditary instincts; this sounds very well, but do you know, my guardians have never been out even to see my mother and father?

Well, on the 18th of January my education began, my mistress teaching me to jump through her arms; no sense to this at all, but as there was a piece of cracker on the other side, I thought I might as well do as she wished. Just at first, I thought her kind, and learned to go upstairs, and on the next day to go down, in order to follow her around. She took me out to walk on the 19th, tied to a chain. I declined to walk in this ignominious way, and when she found that she was simply dragging me along, she seemed ashamed, and letting me go free, I followed her very nicely.

I used to sleep in a room next to the kitchen, but it was horrid in the dark, and I cried dismally. On the 20th it was better, for the cook turned on the light. On the 21st, mistress gave me a ball, which I retrieved very nicely. She also gave me a bath, which I did not like so much, and when she found that I was only a little puppy, and not house-broken, she spanked me so hard that I decided not to have anything more to do with her. I stayed in the kitchen most of the time to let her know what I thought of her. But on the 23rd, she named me "Brother," and, as a little girl came to stay on the 24th, I thought I might as well give in and become sociable again. That day she tried to teach me to jump over her foot. I pretended not to know what she meant, but the next day I did it at once. On the 26th, I learned to shake hands, and I still continue to find that a very pleasant thing to do, as it seems to appeal to humans.

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[3] Not quite three months old.

By this time[3] I was nearly house-broken. Mistress says that I have learned this much more quickly than any of my predecessors. The humans became rather more reasonable, and gave me a basket to sleep in, and so I didn't cry any more on being left alone. On the 28th, I went down town and was so frightened by the crowd, that I decided not to walk. The next day I had another fright. Queer sounds came from a piece of furniture, but when mistress took me in her arms and showed me the piano and its keys, of course I was perfectly satisfied, and have never paid any more attention to the stupid thing. (And the bronze lion scared me too, until I touched it.) I found a very nice hassock to chew, but instead of letting me do this, my mistress threw it at me twice, so it seemed to me simpler to find other things to destroy. By the 30th I was quite an adept at shaking hands, but could not understand the inane difference between right and left. I took my first walk in Mt. Vernon Place on the 31st, and found the fountain very disconcerting and noisy. By the 1st of February, I knew how to sit up and beg, but my back was not quite strong enough to give me any confidence.

After this, I enjoyed my walks, especially playing in the snow, and by the 4th of March[4] I began to realize that there was some method in my mistress' making me do little tricks, and I began to do as she wished. When she says "ta ho" it means for me to stop what I am doing, and when she says "hie on" I may eat my food. On the 15th of February, I learned to "charge." My mistress thinks I enjoy doing these things because I want the reward, but I showed her on the 6th that she was mistaken by doing my tricks right after dinner, and not eating the cracker she offered me.

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[3] Eight days.

[4] A little over six weeks training.

On February 11th, I had a great disappointment, I thought one of my little brothers had arrived, for I suddenly saw something that looked very much like him, and I barked very loudly to show him that I was there too, but it turned out to be only my reflection in a stupid piece of furniture. I won't be fooled by that again, nor had I been before, when my mistress held me up in front of a mirror.

Of course, there were new discoveries every day. On the 14th I was much startled by an open fire on the hearth, and when I found that the house wasn't burning down, I decided to enjoy it instead of barking. On the 16th I ran home ahead of mistress and recognized the house and waited for her on the steps. On another day when I went on alone, she hid behind a corner and I had to go back and look for her. It is curious how many ideas mistress seems to have! On the 20th she began to teach me to carry a letter from one person to another. On February 26, I was the hero of the occasion, for a thief got into the house and I sounded the alarm. The next-door neighbor heard me, but my family were too sleepy to hear, so I gladly ate the soup which the kind thief thoughtfully found for me in the refrigerator. I note in my diary that I weighed 17 pounds on the 28th of February. On that same day, I had three lessons about eating from my mistress' right hand instead of from her left, and in a few days I learned it; perfectly simple, if only you are a little attentive. A little later I was not phazed even when mistress crossed her hands and put them behind her back.[5] I cannot see why children find this a hard trick to do.

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[5] A really remarkable trick, perfectly performed.

On March 15th, I decided to explore, and I ran down the alley, through the side street, and down Park Avenue to the front door. It was locked, and as I could not ring the bell, I had to wait until a neighbor coming by kindly consented to do it for me. I was strong enough by March 10th to learn to jump on a chair, and I was very much teased on that day by having cigarette smoke blown in my face; such a foolish joke for a big boy to play on me.

I used to have keepsakes about this time and would take them to the Morris chair, or the sofa, but since I have grown older, I have given up collecting. I was whipped, after that, for chewing up slippers and books, so that I decided the game was not worth the candle. On March 16th, I began to learn to "die for my country," and on the same day to lose my puppy teeth.

I didn't keep a full diary after this, though I learned to do many things, such as saying my prayers by standing in a high-backed chair, and to understand various signals by my mistress' tone of voice. Strangers think I know their names when I do this trick, but I really wait for my mistress to give me a signal by a rising inflection of her voice, and then I jump and eat the cracker. I enjoy these tricks as much for the applause given me as for the bit of cracker, but that is nice too.

The summer of 1913 and 1914 I spent in Canada. I kept no diary, and I must refer you to the head of the Department for any particulars of my behavior during these two summers. I learned to swim, which was perfectly delightful. I returned to Baltimore in October, 1913, and that autumn my mistress seemed bewitched with the idea of teaching me to do things with a ball (such a horrid, black, smelly little ball it was, too!) I told her that I much preferred a tennis ball, but I haven't thought it worth while to have much to do with any kind of a ball; so from that day to this I retrieve it two or three times for the sake of being agreeable and look for it when it is hidden. And then, after a short time I ignore it, and if the humans upstairs are too insistent, I go down to the kitchen, for I am sure of a welcome there, whether I do my lessons or not. Once or twice my mistress has whipped me when I declined to do my lessons, but I soon showed her that she would get nothing out of me in this way. Sometimes it seems to be a trial between us as to which has the most determination; I generally win out, but I did consent to learn to shut the door. She tried for months by putting a piece of candy on the door-knob, which would fall to my share when the door was closed suddenly by my jumping against it. After months of ill success, towards the end of January, 1914, I at last decided to give in and in three or four lessons became quite expert at it. Now, to my disappointment, however, I no longer find any candy on the knob, but of course I keep on, for I would not have it thought that I do things only for bribes.

Shortly after this, in April of 1914, my mistress became interested in something she called hypnotism, and, looking over my papers, I find the description of how she made me suffer, which I quote:

> April 25, 1914: Made Brother look me steadily in the eye for fifteen minutes. He then followed me as usual into the next room. I made him go "dead dog." He remained quiet for an hour and a quarter, though I moved about, servants entered the room, and I gave stimulus, "country is saved!" When I was forced to go to another part of the house, he rushed down stairs and S——— said, "He does not like the treatment." I then made him come back to the study and "die" again. No lunch for him. Cracker in front of his nose. Eyes open and tail wags. Kept him on chair for some minutes until others came in. He ate a little cracker, but on my telling him to go "dead dog," did not eat. Very nervous when he went down stairs, crouched and trembled.

I leave it to any of my readers to decide whether this treatment was fair, and whether I was not justified in trembling.

I paid two visits to Virginia about this time, and greatly enjoyed going hunting, but I heard that I was not successful in that direction. I only flushed the birds. I have a good scent though, and have found the hunting party when I was separated from them by about half a mile.

By these various trips in summer, it is extremely hard for me to know to whom I really belong, and when my summer master goes in one direction, and my winter mistress in another, I am often placed in a very sad predicament.

I don't feel that I have learned very much in this last year. My mistress tried to teach me to know by name the various objects that she had placed in a row, and would ask me to bring her first one and then another. This seemed to turn me into a common carrier, and I didn't know what it would lead to, and so far I have been absolutely successful in thwarting her plans along this line. The last stunt that has been planned for me is really much the nicest of all. When doing my tricks, I have not been allowed to eat the reward until the signal is given. Now, however, they want to make the test as to whether I have any conception of duration of time. Food is put near me and I am allowed to take it at the end of twenty seconds. This is all very nice when I catch on to the idea, but the old habit is so strong within me that I generally wait for my mistress to give me a nod or a word, and so don't do the trick.

I really understand a great deal that is said to me, and some words have distinct meanings, although more often it is the tone of voice that I am watching for.[6] My greatest pleasure in life is to go for a walk, and when mistress tells me that she cannot take me, I understand and turn tail and go down to the kitchen so that she may not see my disappointment.

I think my development has gone about as far as it can in the house, but I could learn a great many more things out-of-doors. My emotions are also very keen, and I get really angry at what may seem to others slight provocation.

I am now about three and a half years old, and hope to do better work in the future.

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[6] No real evidence of identification of *words* with *objects* was obtained. Words were possibly associated with certain actions: but even here, "expression" of the voice was apparently the important factor.

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EXPERIMENTS ON THE TRANSFER OF TRAINING[1]

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I. INTRODUCTION

It has long been known that if an individual be given training ("practice") in some operation which involves the musculature of one side of the body only, there will be an apparent improvement in the same or similar operations carried on through the corresponding musculature of the other side of the body, although this musculature has not been involved in the "practice." If, for example, the individual is given practice in operating a typewriter with the right hand only, and the practice is continued until a considerable degree of improvement in speed and accuracy is attained, it will be expected that the individual will show now an improvement in the same respects in operating the machine with the left hand, as compared with his performance with the left hand before "practice," although only the right hand has been employed in the "practice" work. This apparent carrying over the "practice" effect from the one side of the body to the other is technically known as *bilateral transfer of training.*

The phenomenon of bilateral transfer is first recorded as observed by Weber (28) and Fechner (9) in the ease with which an individual's unpracticed hand made letters similar to those made by his practiced hand. Volkmann (26) reported similar results in the transference of sensitivity to tactile impressions in distance discriminations. Scripture (20) noted that improvement in steadiness, strength and muscular control was transferable bilaterally. Davis (6) reported the results of tests and practice in lungeing, and with dumbbells, tapping, dynamometer and ergograph; and from his data concludes that "the effects of exercise may be transferred to a greater or less degree from the parts practiced to other parts of the body. This transfer is greatest to symmetrical and closely related parts." Wissler and Richardson (32) concluded from data gathered by experiments with a hand dynamometer, that large transfer of training accrues to the unpracticed hand. "The accessory muscles of one side gained approximately as much from the exercise of the corresponding muscles of the opposite side as from the exercises of the fundamental muscles of the same side." Wallin (27) trained observers in monocular control of the illusive phases of reversible perspectives. The improvement that resulted from the training of one eye was shared by the untrained eye.

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[1] The experimental work reported in this paper was done for the greater part at the Johns Hopkins University during the years 1914–1916, and completed at Dickinson College during the year 1917–1918. An extensive historical summary which had been prepared was lost, during the author's absence in the army, through no fault of his. In its present form, the paper was completed May 1, 1920.

Swift (22) concluded from his experiments in tossing two balls, that in the majority of cases the training of the right hand was effectively transferred to the left hand. In one case, in four days of practice the left hand excelled the attainments of the right hand previously practiced eleven days; the transfer being perhaps due to the "content already learned." Hill (11) found positive evidence of transfer of training from the practiced to the unpracticed hand in his experiments on mirror-drawing of a star.

The phenomena observed have been given various tentative explanations, mostly in terms of physiological theories. The problem cannot, however, be divorced from the more general problem of transfer of training, which is included in the much debated topic of "formal discipline," which is the supposed training of abilities in one subject, study or direction, in such manner that the abilities may be applied effectively to other studies or in other directions. Under this topic belong the hypothetical "training of memory" and of other "faculties," and the "training of the mind" through the study of a subject such as Greek or mathematics in the hope of making thereby the mind more efficient in other work.

On the subject of "formal discipline" or "transfer" in general, the investigations have been numerous and the literature is extensive. It is not necessary nor desirable in this report to give a complete summary of the same. Only a few of the more striking examples will be mentioned here.[2] Bennett (2) reported transference of the effect of training the memory in learning poetry, to learning of rows of digits and lists of names; Ebert and Meumann (8) in a elaborate study of the influence of memory-training in learning nonsense-syllables, tested by memory work in numbers, poetry, optical designs and word pairs in German and Italian, reported in favor of effective influence of this special learning upon the tests; Dearborn (7) in repeating these tests with controls, obtained data from which he inferred that much of the improvement in the test is, in fact, to be considered as due to the special training. Fracker (10) reported that a course of training in memorizing a series of tones of varying intensities results in marked gains in memory tests in memory for poetry, colors (grays), pitch, geometrical figures and muscle movements; Winch (31) reported gain in ability to memorize history as a result of practice in memorizing poetry. Rall (16) found evidence of transfer of training resulting from miscellaneous memory work twenty minutes a day carried over to tests in memorizing "Evangeline" and nonsense syllables. Sleight (21) found transfer from memory training in poetry to memorizing of nonsense syllables; from training in memorizing tables to the memorizing of dates; and from training in memorizing prose to memorizing prose and names; Thorndike and Woodworth (23) reported transfer of improvement resulting from a training course in estimating areas in rectangles, of from ten to one hundred square centimeters in area, over to the estimating of areas of various sizes and shapes. Judd (12) in his investigation of the Müller Lyer illusion under varied conditions, found evidence that the effect of the training in estimating the error of the illusion in one form of presentation is transferred to estimating the error in another form of presentation; Ruediger (17) found possible the transfer of general ideals, e.g., of neatness, from one school situation to another; Lewis (14) found in his study of the Müller-Lyer illusion that after continued practice in one form, the illusion may be made to disappear, and when the illusion plate is subsequently presented in another form, the illusion will reappear, but may be overcome more quickly by a training course in auditory discrimination; Angel and Coover (1) found that the power of discrimination of brightness and color is increased, and that typewriting becomes speedier and more accurate, following a training course in card sorting. They found also that training in sorting cards under one set of conditions produces an improvement in the sorting cards under different conditions. Pyle (15) and Saxby (19) found that training in the quick perception of numbers and in the observations of specified objects produced positive improvement of general observations: and that training in ideals of accuracy and neatness in one kind of work may be effective in temporary transfer of those ideals to other kinds of work. Coover (5), in experiments testing sensory discrimination, attention, muscular coördination, memory, discrimination and choice, claimed full justification for stating that skill gained in specific exercises may be available for general use.

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[2] For a very complete résumé of the literature on the subject of Transfer, the reader is referred to Coover, Formal Discipline, Psychol. Rev. Mon., vol. xx, no. 3. Most of the bibliography presented in this report was read, as well as the work of the investigation done, before the monograph by Coover was read.

II. EXPERIMENTS ON THE TRANSFER OF TRAINING

The experimental work herein reported was conducted principally on bilateral transfer from one hand to the other, with subsidiary experiments designed to secure material for the interpretation of the bilateral effects. The apparatus consisted essentially of a Burroughs adding machine, subtractor model, ten column, motor driven. Certain devices described below were supplementary to this machine. The work was done in the "middle four" columns, the other columns being cut off. At the top of each column is a release button, pressing which releases any digit buttons depressed in that column, and makes possible the correction of an error, should such be detected before printing. At the right side of the keyboard of the machine is the *error button*, pressing which releases all the digit buttons depressed. The printing operation was accomplished by a single movement of pressing the *print bar* situated at the right hand of the keyboard. Instructions were that no attempt at correction was to be made by the reactor after the number had been printed, but there were no restrictions set upon using the release buttons at the head of the digit columns, or of the error button prior to the printing operation. This feature of the manipulation of the machine was usually learned without difficulty.

The adding machine presents a relatively simple problem to a novice. The keyboard is learned almost at a glance. One need not get lost looking for the proper "button" or "key," as is frequently the case with a beginner at the typewriter. The adding machine also makes it possible to work with either hand with almost identical movements. The only asymmetrical movement required in these experiments was that made by the left hand in pressing the print bar. This was a movement across the entire body, since the print bar was attached on the right side of the machine. The right hand could press the print bar by moving from the center of the machine to the right of the machine; not a cross body movement.

Number-sheets as work units for the adding machine are of more uniform difficulty than word-lists for the typewriter, numbers probably require a more uniform effort for recognition than do words. Accumulative memory products are also not so probable when number sheets are used as they are when word lists are used on a typewriter.

The numbers on the work sheets comprise the "four-place" number addition system of the Johns Hopkins Psychological Laboratory, and will be printed separately later. These work-sheets consist of ninety four-place numbers, derived by a system such that no number shall have zero as a final digit, and with the distribution of the various digits such that each sheet is exactly as difficult as any other; and yet the numbers are so varied that there is no detectable memory complication in reading the sheets. For convenience in reading, the sheets were cut into work strips of forty-five four-place numbers each, since a list of ninety numbers is not easily handled on the work table. The totals in time and accuracy are, however, in the units of the whole work-sheet with the single exception, section 2, page 358, where the fastest third is considered. In this case, the unit is the *work-strip*, i.e., half the work sheet, having forty-five four-place numbers in it, on which unit the kymographic record was made.

Since the ordinary work-table of the left side of the adding machine was unsuited to this test, a special one was provided. On a pair of upright rods, supported by heavy iron pedestals to keep them firm, a rectangular frame was placed around the machine at the height of, and inclined to the same angle as, the keyboard, and extending 12 inches on each side of the same. Sheets of aluminum were fastened to this frame, fitted tightly on each side of the machine at the height and angle of the keyboard. On these aluminum sheets was fastened heavy cardboard of neutral gray color, to prevent reflection.

Around the machine was placed a screen covered with a dead black cloth so that no reflection from the sides of the room could reach the reactor. The keyboard and work-table were lighted by a 40-watt Mazda bulb, hanging immediately above the machine. Uniform conditions of lighting were thus maintained. The maximal speed of the adding machine was forty-five printing operations in thirty-one seconds. The machine was tested at irregular intervals throughout the course of the experiments and this speed was always maintained.

Each reactor was given specific and uniform instructions, including illustrations of the manner of putting numbers into the machine. Any question asked was fully answered before the work began, but no practice was allowed prior to the test. Each reactor was told that he would be graded for speed and accuracy. No reactor was informed during the progress of the experiment as to his proficiency. A few of the reactors knew they had made errors, because they had seen that they had pressed the wrong digit button, but could not inhibit the movement to press the print bar in time to prevent printing the error. The totals were taken by the experimenter, the reactors doing the listing and printing only. By "listing" is meant setting on the keyboard of the adding machine the numbers of the work sheets. The time was measured by a split-second stop-watch.

The work strips were fastened to the cardboard cover of the work table on the side opposite to the hand which was to be practiced. The "idle" hand was used by the reactor to follow the items of the work strips.

The first day's work for each reactor (the "preliminary tests") consisted of an equal number of sheets to be listed by each hand. The first training series was usually nine or ten one hour periods, distributed over about three weeks, and but one hand was used on the machine in this practice period. A "semi-final" test was then given to each hand and usually this semi-final test was followed by a practice period for the "idle" hand of the previous training period. The series then concluded with a "final" test of each hand. This scheme was varied in some instances which will be noted in the proper places.

After the first day's work (the preliminary test), the work strips were started on a four-minute "head-way;" i.e., the second strip was started four minutes after the starting of the first, the third was started four minutes after the starting of the second, and so on; consequently, as the time required for each strip decreased, the lengths of the rest-periods increased. In some few cases, towards the close of the training the work periods were a little less than half the total time of the day's series.

The work of the several reactors may be grouped as follows: A: *Simple listing,* by which we mean reading the numbers from the work sheets and putting the numbers read into the machine. B: *Observation,* which means watching at close range other reactors working at simple listing under conditions described in section B. C: *Auditory listing,* when the reactors put into the machine numbers from the work sheets read to them by the experimenter. D: *Number reading training,* when, under condition detailed in section D, the reactors practiced reading the numbers of the work sheets but did not operate the machine.

E: *Machine training,* where reactors practiced on the machine but did not have work sheets. Detailed description of conditions of this group is given in section E.

Section A. Simple listing. Six subjects, H., Sg., Lo., E., Cl., Mc., worked at simple listing. Table 1 gives in condensed form the results of this work (Plates I–VI).

TABLE 1

Improvement in simple listing

Semi-final tests

REACTORS	PER CENT OF IMPROVEMENT OF WORKING HAND	PER CENT OF IMPROVEMENT OF IDLE HAND	ORDER OF TESTING HANDS	
			Preliminary	Semi-final
H.	R. 16	L. 14	L. R.	R. L.
Sg.	R. 41	L. 32	L. R.	R. L.
Lo.	R. 25	L. 20	L. R.	R. L.
E.	R. 40	L. 48	L. R.	R. L.
Cl.	R. 50	L. 29	R. L.	R. L.
Mc.	R. 25	L. 26	L. R.	R. L.

Final tests

REACTORS	PER CENT OF IMPROVEMENT OF WORKING HAND	PER CENT OF IMPROVEMENT OF IDLE HAND	COMPARISON*
H.	L. 20	R. 3	5*
Sg.	Not completed	Not completed	5*
Lo	L. 5	R. 2	3†
E.	L. 16	R. 10	‡
Cl.	Not completed	Not completed	
Mc.	L. 13	R. 7	5*

* Number of days required by working hand following the semi-final test to equal record on semi-final test of hand previously working.

† Unpracticed hand (L.) in semi-final test reached level requiring four days of practiced hand, and in three days reached level requiring eight days work of right hand to equal or surpass.

‡ In two days the unpracticed hand (L.) reached level requiring practicing hand six days to reach.

In every case the unpracticed hand improved over its former test from 14 per cent (H.) to 48 per cent (E.). In two instances the percentage of improvement in the unpracticed hand exceeds that of the practiced hand (E.), (Mc.). After this semi-final

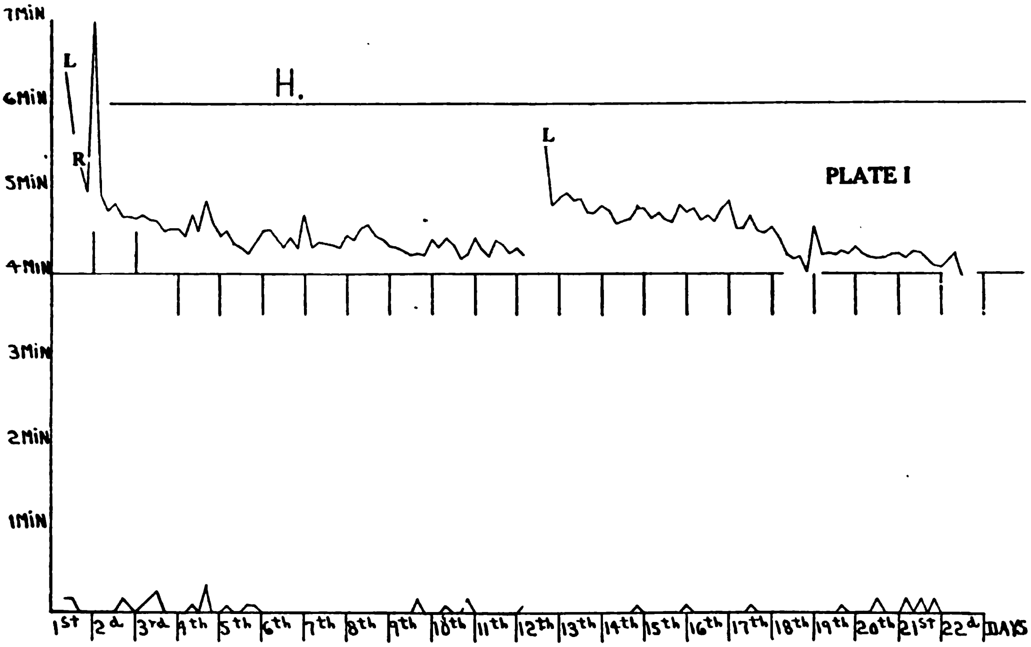


PLATE I

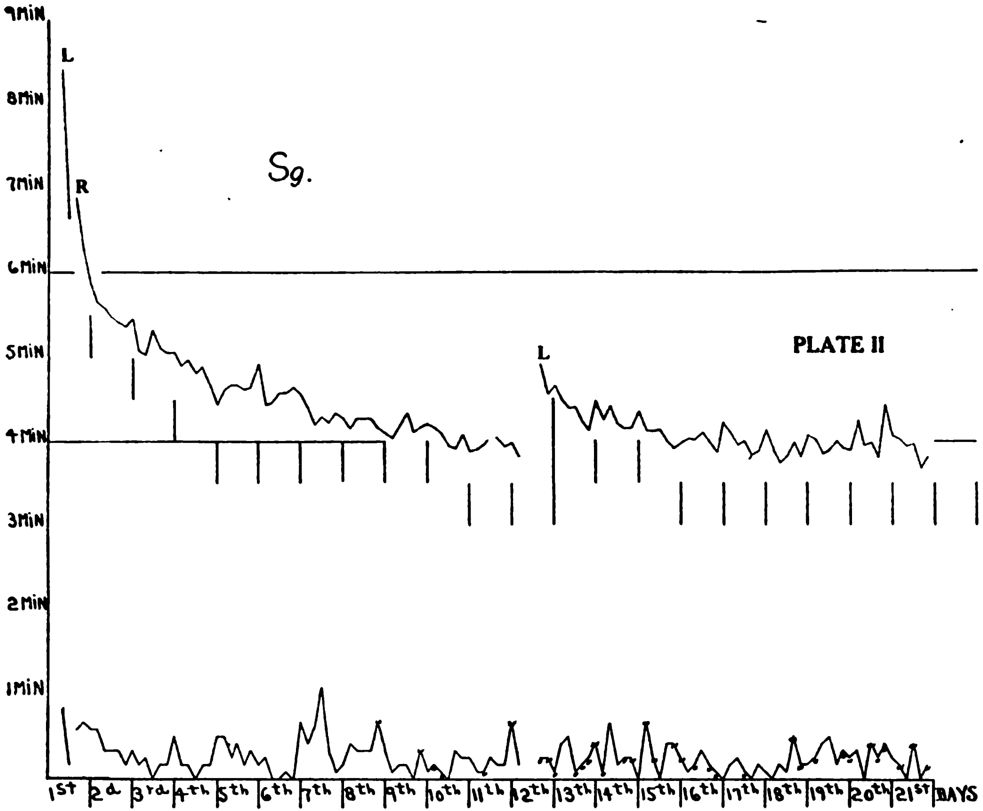


PLATE II

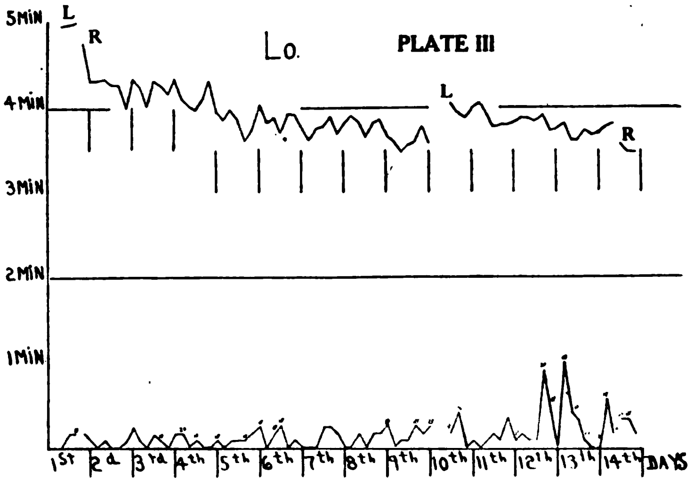
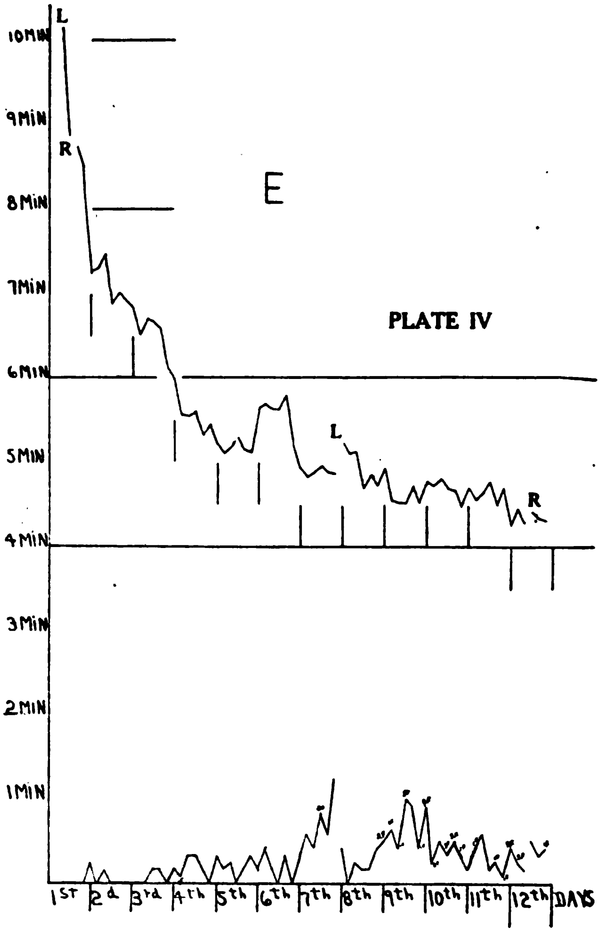


PLATE III



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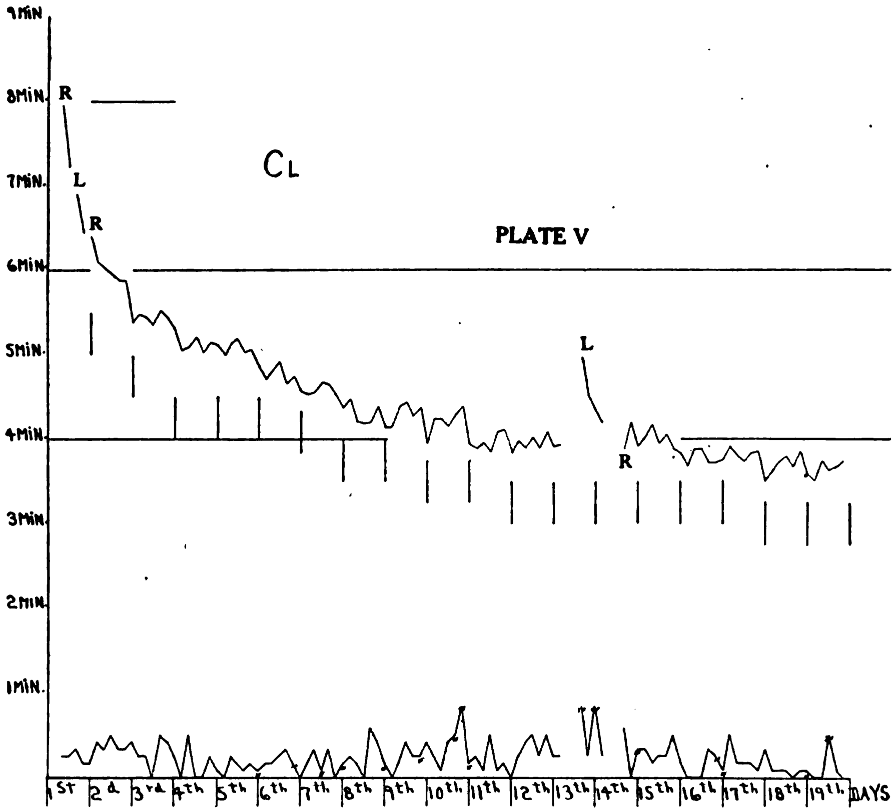

PLATE V

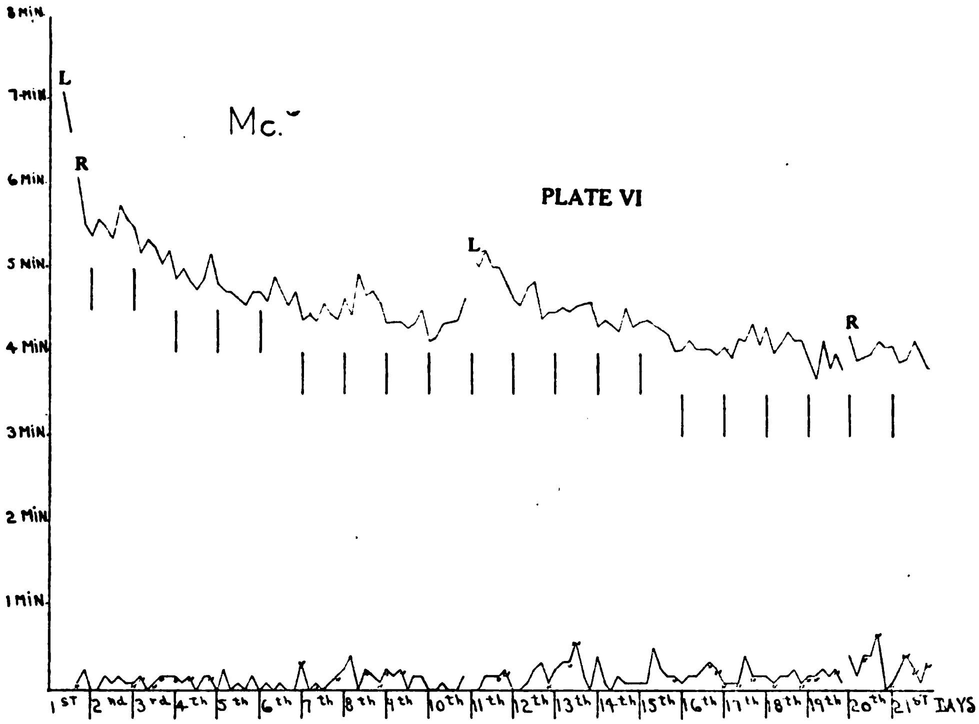

PLATE VI

test, four reactors continued the experiment, working the hand which was "idle" during previous training test. The results of this training appear with positive evidence for improvement in both hands. The percentages of improvement for the "idle" hand range from (Lo.) 2 per cent while the working hand gained 5 per cent, to (E.) 10 per cent for the "idle" hand while the working hand gained 16 per cent. The percentages indicate the decrease in the amount of time required to do a fixed amount of listing.

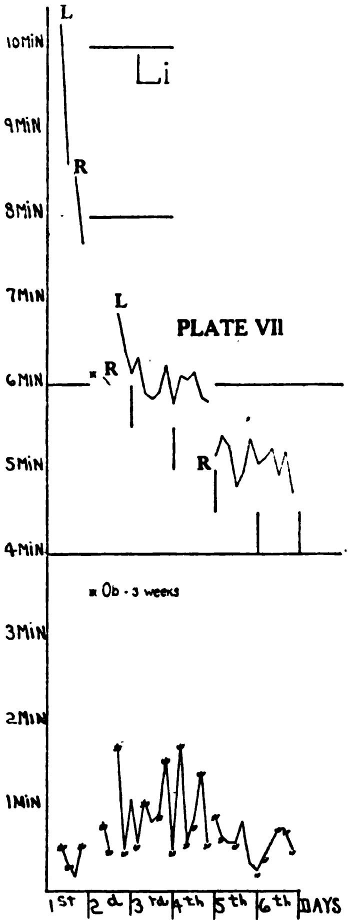

PLATE VII

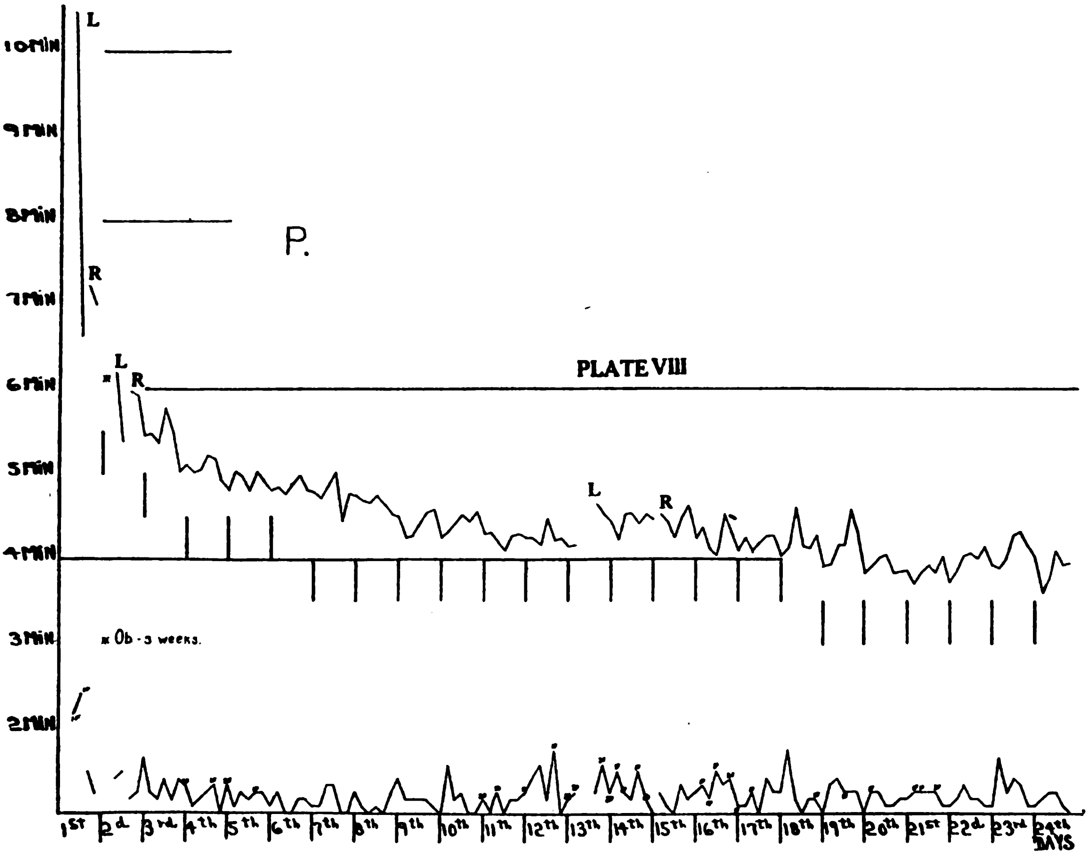

PLATE VIII

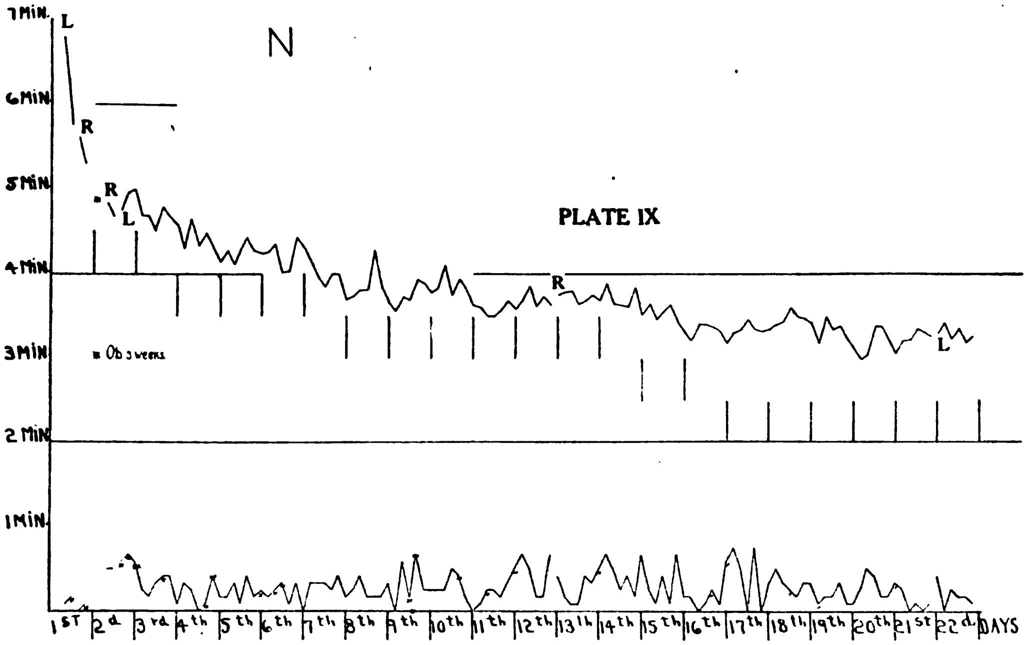

PLATE IX

Section B. Observation with training in number reading. There are two major processes in simple listing on an adding machine; (a) reading the numbers and (b) putting the numbers into the machine. The first of these processes is a common factor in working either hand, and improvement in the ease and accuracy of reading the numbers may be the medium of apparent transfer of training evidenced in the increase of speed shown by the semi-final trials of the unpracticed hands. To test the practice value of the number-reading process, reactors Li., P., N., Wm., We. and F. (Plates VII–XII), were each given the regular preliminary test and then served as observers for periods as described below.

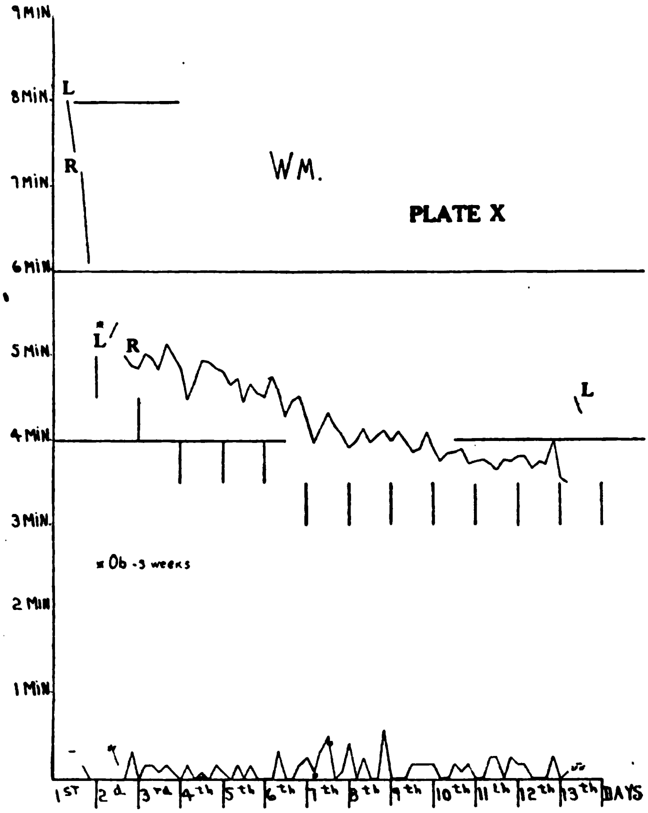

PLATE X

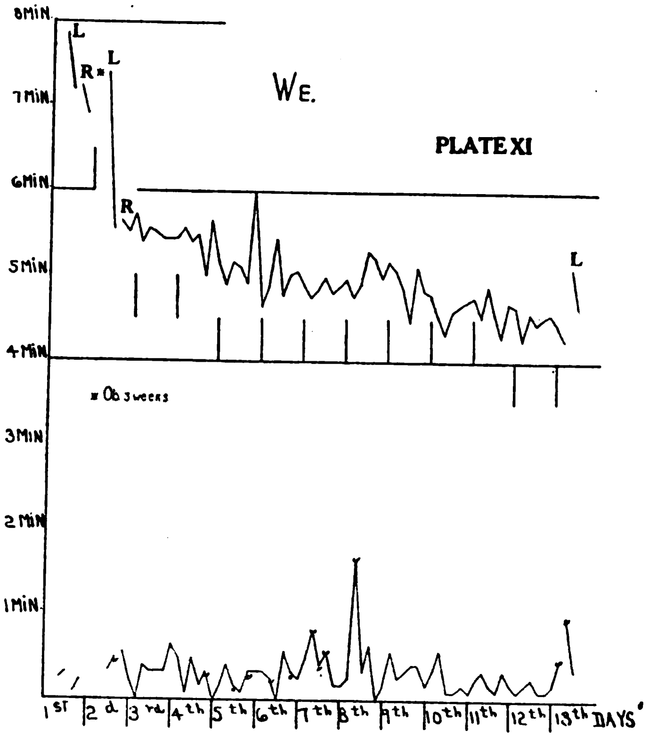

PLATE XI

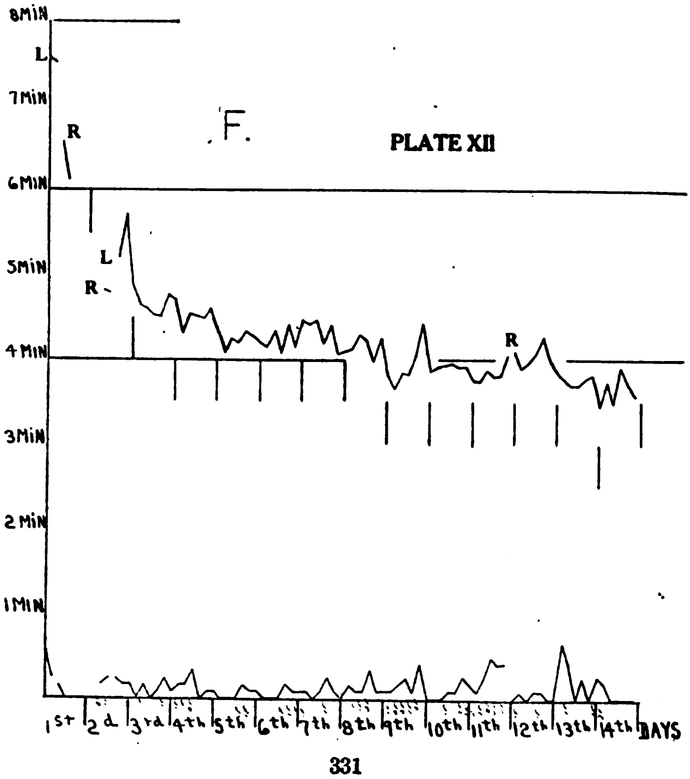

PLATE XII

The "observers" were seated beside the operator near enough the work sheet to read the numbers accurately and as rapidly as the operators. They were able also to observe the digit buttons as they were pressed by the operator. They were instructed to make note of any errors seen, and to report the same privately to the experimenter at the close of the period. A few errors were correctly reported by Wm., and P. N. and F. held in their hands a sensitive rubber bulb, connected by rubber tubing to a Marey tambour which controlled a stylus on a kymograph: and they were instructed to press the bulb gently when an error was noticed. The records show careful attention on the part of the observers and a high degree of accuracy as measured by the errors detected and recorded. It is possible that those observing did not attend to the reading of the numbers with the same degree of care as they would have exercised had they been operating the machine, but they did undoubtedly have considerable practice effect from the reading of the numbers. The sensitive bulb held in the hand to mark errors did not give any record on the kymograph which could be fairly interpreted as evidence of implicit practice of the fingers.

Table 2 contains the number of hours of observation and the percentage gained in listing as shown by the semi-final test. In these cases there is a gain in speed ranging from 13 per cent (N., right) to 31 per cent (Wm., left). There must be taken into account, in appraising the value of these percentages of gain in speed, the order in which the hands were worked in the preliminary and semi-final tests. If the left hand is the first one worked in the preliminary test, then the right hand has the practice value of that period of working, and *vice versa*. If the hand working first in the preliminary test works last in the semi-final test (i.e., the order L. R., R. L.,) then that hand shares the advantage of the two practice periods of the other hand. Three of the six reactors worked in the order L. R., L. R., viz., reactor P., whose percentage of gain of each hand was practically the same, reactor Wm., whose left hand gained five per cent more than the right hand, and reactor We., whose right hand gained 8 per cent more than the left hand. The cases working the order L. R., R. L., show the largest gain to have been made by the hand which worked first and last, suggesting the practice effect of the two working periods of the other hand as well as the preliminary period of its own.

TABLE 2

REACTORS	HOURS	PER CENT GAINED		ORDER OF HANDS	
		Right hand	Left hand	Preliminary	Semi-final
P.............	9	30	30	L. R.	L. R.
N.............	9	13	23	L. R.	R. L.
F.............	9	24	25	L. R.	R. L.
Wm............	10	26	31	L. R.	L. R.
We............	9	22	14	L. R.	L. R.
Li.............	6*	25	29	L. R.	R. L.

* In seven weeks.

TABLE 3

	F.		Wm.		We.	
	Working hand	Idle hand	Working hand	Idle hand	Working hand	Idle hand
Total gain..................	47	45	47	43	38	36
Gain by observation.......	25	24	26	31	22	14
Gain by work..............	22		21		16	
Gain by transfer...........		21		12		22

	N.*	
	Left hand	Right hand
Total gain.........................	47	40
Gain by observation...............	23	13
Semi-final..........................	18 (working)	19 (resting)
Intermediate.......................	6 (resting)	9 (working)

* Both hands were worked at simple listing after observation with semi-final, intermediate and final tests.

Following the period of observation, four of those who had acted as observers continued the work in simple listing. The evidence of improvement to the "idle" hand in the results of their continued listing are seen in table 3. Reactors F., Wm., and We., worked only one hand after the period of observation and took the final test. Reactor N. worked each hand after the observation period with intermediate and final tests. Table 3 shows the comparative results of work and observation.

The relative value of observation as compared with practice is shown in table 4, presenting total percentage of improvement, and percentage of improvement during observation. These percentages of improvement and total improvement are based on the preliminary tests and are therefore absolutely comparable. Wm., and We., did not practice the left hand at all, the percentages of total improvements of the left hands of these two reactors being due to observation and practice of the right hands. The left hand received only the preliminary, semi-final and final tests. In every case, except N. (right hand), the amount of improvement during observation exceeded the amount of improvement following subsequent practice, indicating the value of the training in reading the numbers.

TABLE 4

	Li.		Po.		N.		F.		Wm.		We.	
	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.
Percentage of total improvement............	37	37	45	49	40	47	45	47	47	43	38	36
Percentage of improvement by observation..	25	29	30	30	13	23	24	25	26	31	22	14

The mean gain in percentages in simple listing shown by the semi-final tests is: For the working hand (R) 32⅔ per cent; for the "idle" hand (L) 28⅓ per cent.

The mean gain in percentage by observation shown by semi-final tests is: For right hand 23 + per cent; for left hand 26⅔ per cent.

The mean percentages of gains of the right hand of six reactors working nine to ten days is only 9⅓ per cent more than the mean percentage of gains of the right hands of the six reactors who observed them those nine to ten days; and the mean percentages of the "idle" hands of these same six reactors working at simple listing is 1½ per cent more than the mean percentage of the gains of the left hands of the six reactors who acted as observers for those nine or ten periods of work.

Section C. Auditory listing. To give the hand practice in putting into the machine the numbers, and at the same time not train the number-reading process, reactors S., T., W., K. and C. (Plates XIII–XVII), after the regular preliminary tests, put the numbers into the machine as they were read to them by the experimenter. Care was taken not to read the numbers too rapidly, and also not to allow any rest between the numbers. The rule followed by the reader was to begin to read as soon as the print bar had been touched. The reactors were instructed to ask for a re-reading if the number was not clearly understood, and this was done in relatively few cases. Generally speaking, there was no difficulty experienced in reading the numbers clearly

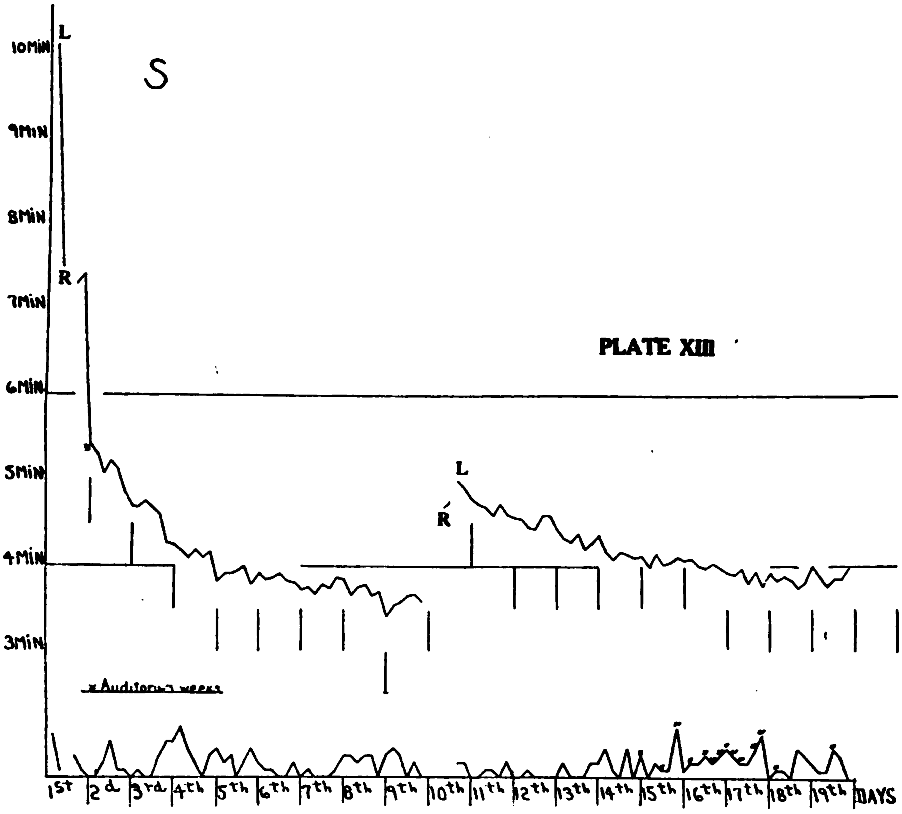

PLATE XIII

and with sufficient speed to keep the reactors working regularly. The results of this auditory training and of the regular semi-final and final tests appear in table 5.

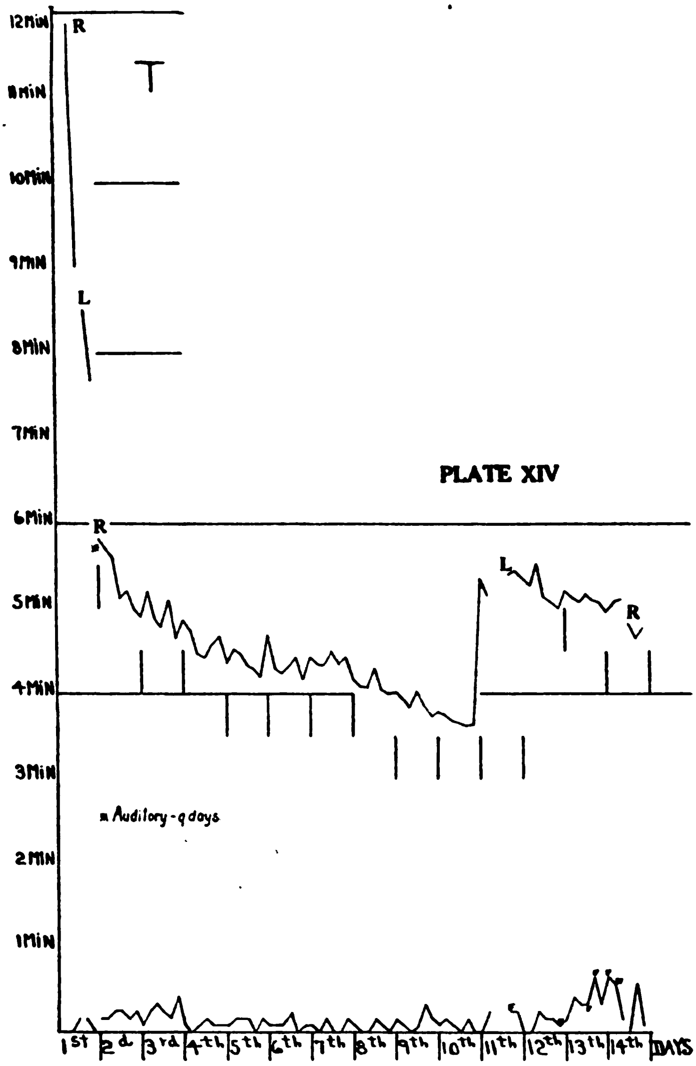

PLATE XIV

In four out of the five auditory listing cases, the idle made greater gains in the semi-final tests than did the working hand, regardless of hand and order of working of hand in preliminary or semi-final test. In no case did the practice in the regular method of working the machine following the auditory listing result in as rapid work as that done in the auditory listing. In simple listing, there are present several minor processes which are absent in the process of auditory listing: i.e., (a) keeping one's place on the work sheet; (b) turning one's head to read the numbers; (c) turning back again to see the keyboard; (d) carrying the numbers read, back to the machine, and (e) identifying the digits read and the proper buttons to be pressed. After practice of considerable length, one may learn to read the entire number, i.e., the four digits, as a single group without separating the group into its individual parts: and the process of putting those digits in the group into the machine may become a "group movement:" that is to say, while it will always require the pressing of the various buttons, the attention is given to the whole set of necessary movements as one movement. It is as if the numbers at first were read and listed 1–4–7–3, but after considerable practice, the reactor learns to read the numbers 1473, and his impulse to list them is one impulse for the four digits as of one number, 1473. This is a more marked characteristic of some reactors than of others. The reading of the numbers at first requires at least two or more glances. Reactor Pe. read the numbers as groups of digits and listed them as groups. The number 1473 was read and listed 14–73, requiring two glances and two distinct operations. It is true that Pe. was more inclined

TABLE 5

Showing percentage gains of auditory listing

REACTORS	NUMBER OF HOURS AUDITORY	PER CENT GAINED		ORDER OF WORKING HAND	
		Right hand	Left hand	Preliminary	Semi-final
S.	8	36*	47	L. R.	R. L.
T.	9	49*	37	R. L.	R. L.
W.	9	8*	12	R. L.	R. L.
K.	9	29	17*	R. L.	R. L.
C.	9	20*	34	L. R.	R. L.

* Indicates hand working during auditory listing.

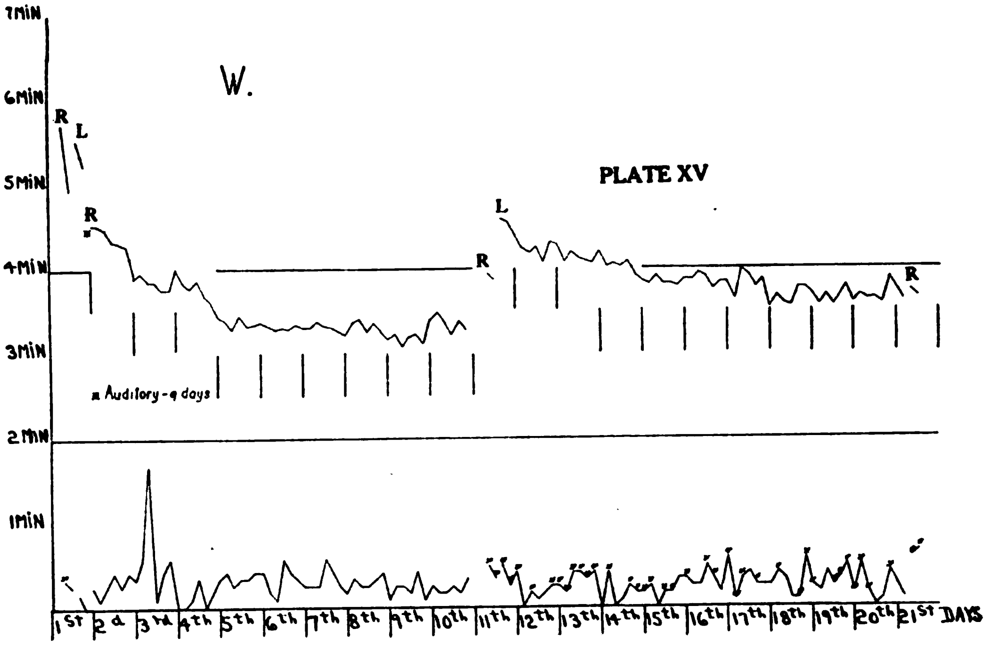

PLATE XV

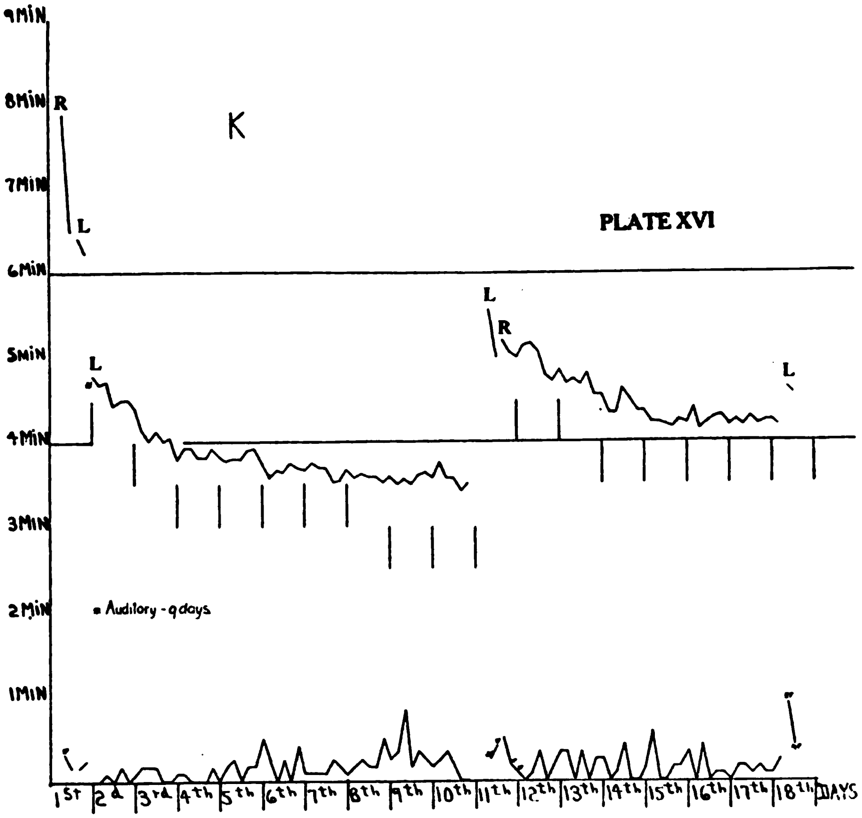

PLATE XVI

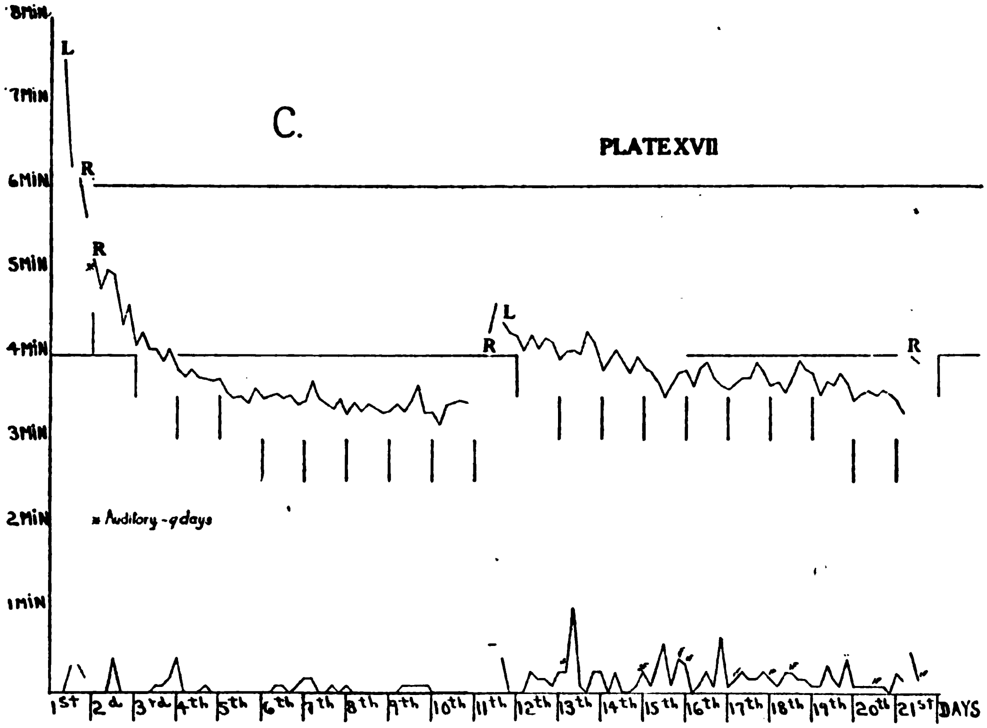
PLATE XVII

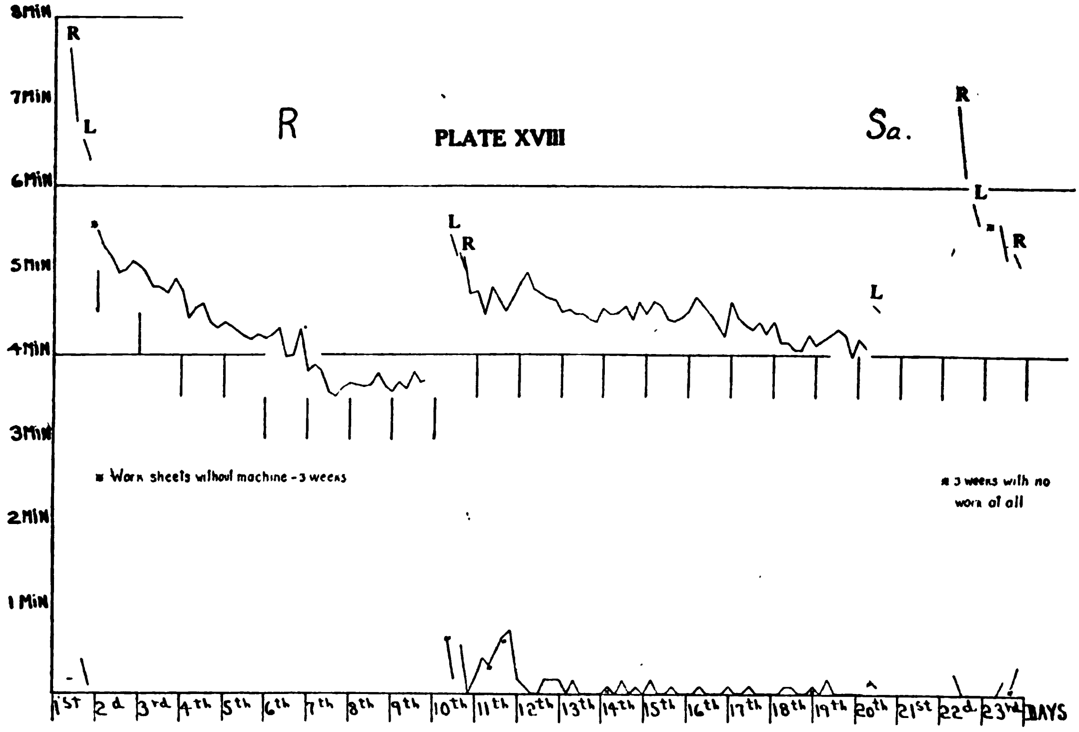
PLATE XVIII

to this method than any other reactor, and yet it was characteristic of all the reactors in the beginning, with the exception of Lo., who had used the machine for a few hours some months prior to taking the preliminary test. Toward the end of the series, every reactor learned to read the number at one glance, only occasionally taking the second glance. The process of reading separate and individual digits became a process of reading a unified group of digits. It is probable that the process of listing is not so perfectly unified, because the practice periods did not extend over a sufficient time. Statements of the reactors would indicate that the listing was not a perfect "group movement," and the experimenter, who gave close observation to the work throughout, is of the opinion that even the best reactors did not attain more than partial unification of four-place listing. This was made evident by the arhythmic listing and by the frequent pauses and periods of confusion. In auditory listing, the process is simpler, for the elements noted above (a), (b), (c) and (d) are not present. Attention is not diverted from the keyboard. As soon as individual digits are heard, they are put into the machine, and after some practice, it is not uncommon for a reactor to have listed the third digit in a four-place number by the time the last digit of the number is read, i.e., the reactor is listing only one digit behind the reader. In a limited period of practice, therefore, better time can be made in auditory listing than in simple listing, if the reader is clear voiced and reads with adequate speed.

It is possible that the interference between auditory listing and simple listing accounts for the fact that in every case but one, the idle hand during the auditory listing made better percentage of gain at the semi-final test than did the working hand of the training series.

Following the auditory listing and semi-final test, practice in simple listing was given to the idle hand of the auditory series, with the results set forth in table 6, in which the percentage of gain is calculated upon the basis of the time of the preliminary tests.

The percentage of gain of the idle hand in the simple listing process is less than that made by the practiced hand in three out of five cases, equals it in one case, and surpasses it in one case. In all cases, the idle hand makes improvement ranging from 4 per cent (T., having only two hours practice) to 24 per cent, (W.). The gains of the practiced hand were from 4 per cent (reactor T., two hours) to 16 per cent, reactor W.

Section D. Number reading without machine practice. To discover, if possible, the avenue of the so-called transfer, two reactors, R., and Ir. (Plates XVIII and XIX), were given the regular preliminary tests, and then, sitting at the machine, right hand on the work sheet to hold the place in the column of numbers, in the regular position for working, they were directed to read each four-place number on the work sheet, looking at the keyboard to identify each number without touching the buttons. This gave practice to the visual processes in the performance of the regular work of the machine, but gave no manual or digital practice. The regular number of sheets was done each day and timed as usual. It was, of course, impossible in this phase of the work, to check for accuracy. The reactors worked very faithfully and diligently, and the experimenter is of the opinion that the visual processes were used in this training series fully as much as in the regular operation of the machine. Each of the reactors used the lips in reading the numbers throughout the entire test, and in this training course, they seemed to accompany the recognition and location of the appropriate digit buttons with a characteristic nod of the head. The semi-final test in simple

TABLE 6

REACTORS	HOURS OF WORK	PER CENT GAINED		ORDER OF WORKING HAND	
		Practiced hand	Idle hand	Preliminary	Final
S..............	9	13 L.	11 R.	L. R.	L. R.
T..............	2	4 L.	4 R.	R. L.	L. R.
W..............	9	16 L.	24 R.	R. L.	L. R.
K..............	6	12 R.	10 L.	R. L.	R. L.
C..............	9	15 L.	8 R.	L. R.	L. R.

listing followed this course of training in number reading. Table 7 shows the results of this training.

The practice in reading the numbers and locating them on the keyboard resulted in gains to both hands with both reactors. The hand which worked first in the preliminary and last in the semi-final tests, in both cases the right hand, made greater gains.

Following this number reading training, the reactors took a final practice series in simple listing, the right hand working in each case, the results of which are given in table 8, percentages in which are based on preliminary time records, not on semi-final record. In both cases, there was improvement in both hands, the idle as well as the practiced hands.

TABLE 7

REACTORS	HOURS OF NUMBER READING	GAIN		ORDER OF WORKING HAND	
		Right hand	Left hand	Preliminary	Semi-final
R.	8	31	18	R. L.	L. R.
Ir.	9	31	23	R. L.	L. R.

TABLE 8

REACTORS	HOURS WORKED	GAIN		ORDER OF WORKING HAND	
		Practiced hand	Idle hand	Semi-final	Final
R.	9	11	12	L. R.	L. R.
Ir.	7	17	11	L. R.	L. R.

Section E. Working machine without "work sheets." Three reactors, A., Pi., and Pe. (Plates XX–XXII), after the regular preliminary tests with both hands, were worked on the machine putting four-place numbers into the machine without "work sheets" from which to read. The instructions were (1) to use any number they could think of; (2) varying the digits as much as possible; (3) covering the entire working keyboard, (4) always deciding upon the entire number before pressing any buttons, and (5) using no zero in the units column. The reactors worked diligently and conscientiously. There was, of course, no chance to check their work as to all points on which instruction was given, but the numbers were well scattered as to selection, and as varied as could be expected. After this series of practice periods, working the machine without work sheets to be read, the semi-final tests in simple listing were made, yielding the results showed in table 9. Gain was made by both hands of

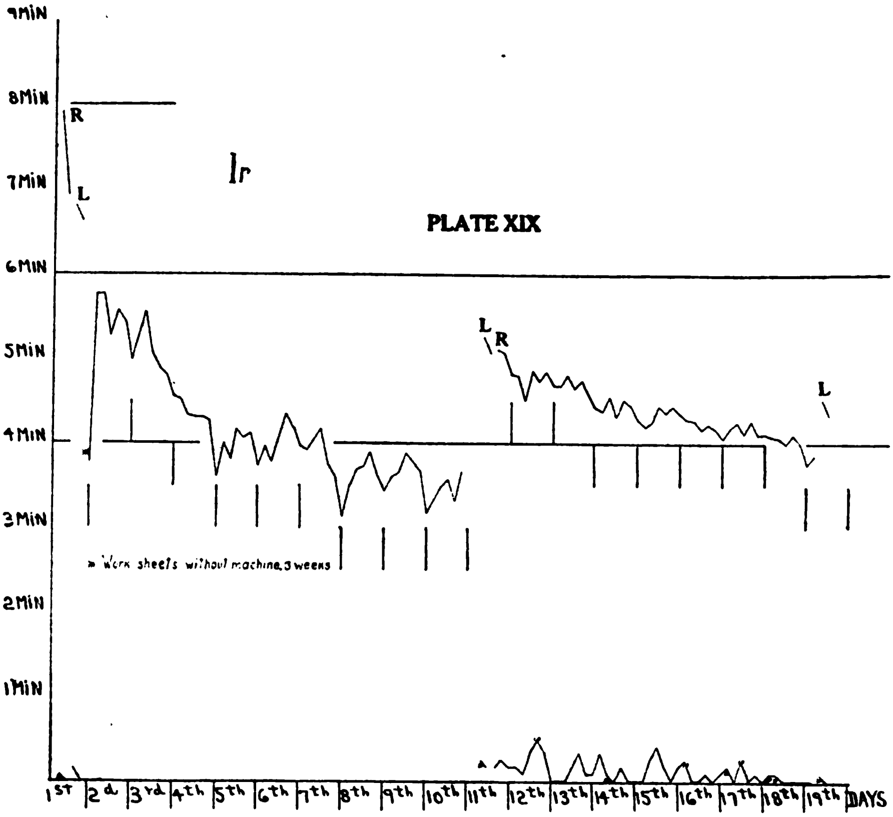
PLATE XIX

every reactor. For reactor A., the gain of the practice hand was nearly four times as great as that made by the "idle hand;" for Pi., and Pe., the gains made by the idle hands were greater than the gains made by the practiced hand. The order in which the hands were worked in the preliminary and semi-final tests has significance in the understanding of these figures. For

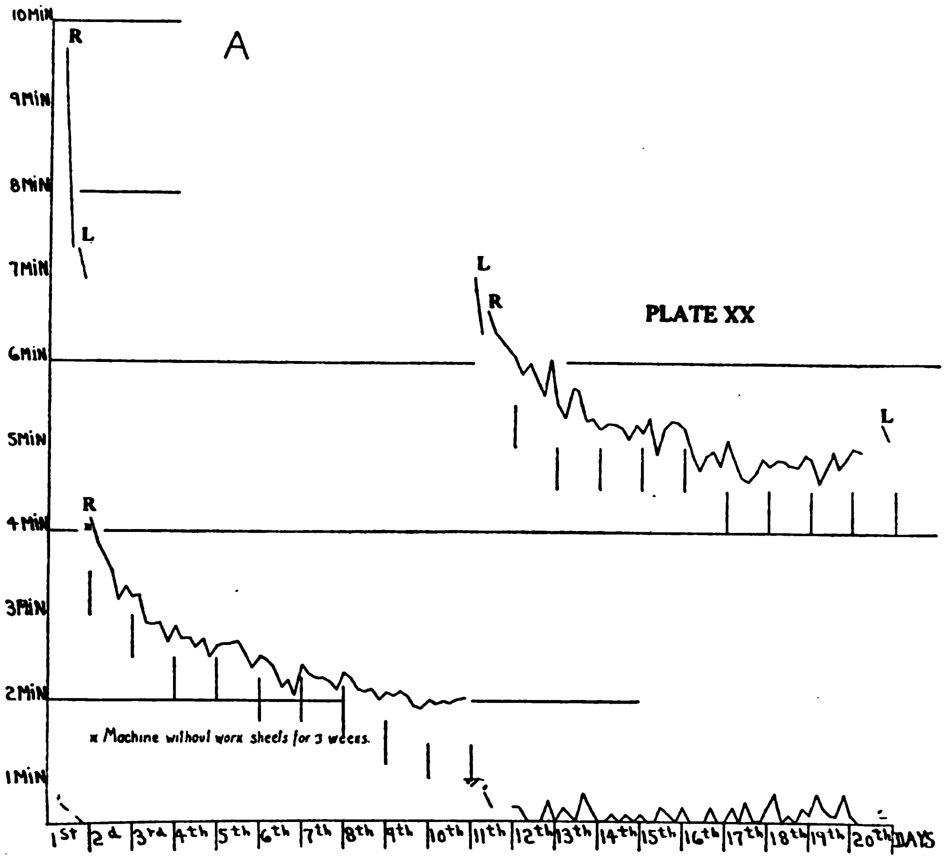
PLATE XX

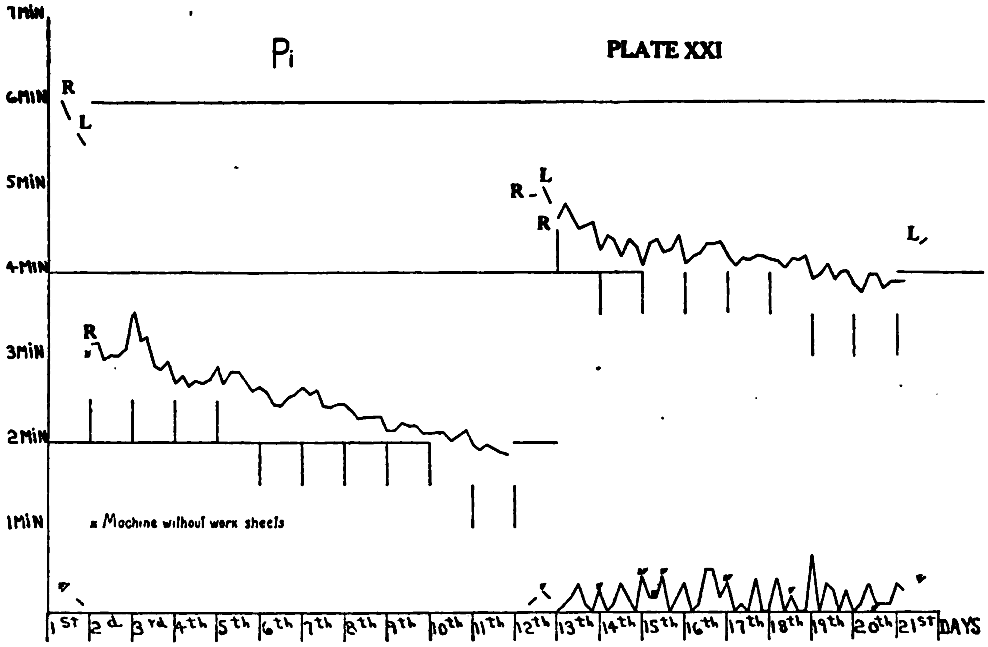
PLATE XXI

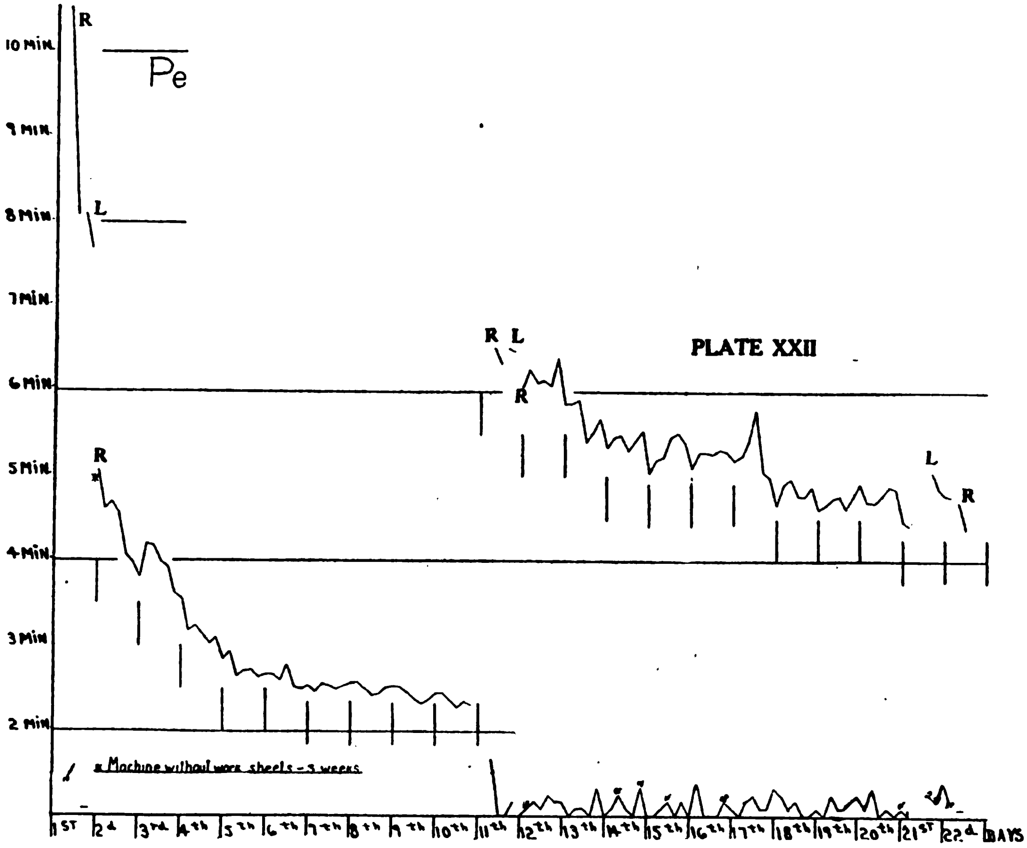

PLATE XXII

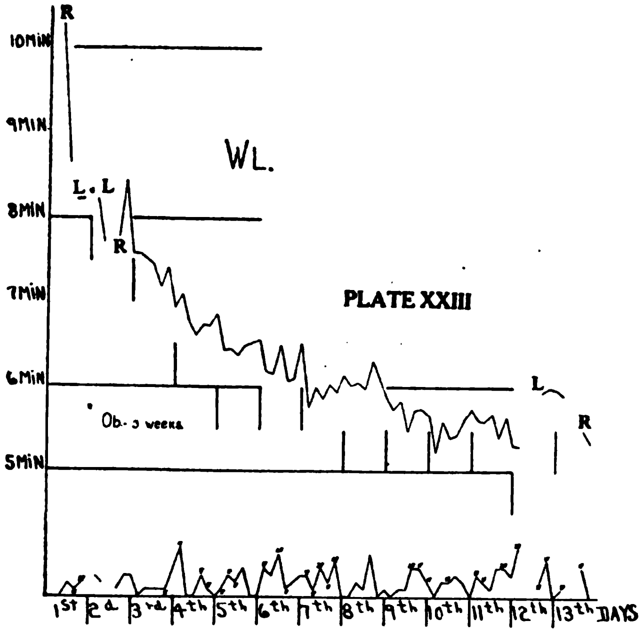

PLATE XXIII

reactor A., the order of working the hands was R. L., L. R. The right hand, being first in the preliminary test, made the poorest record, as would be expected. The left hand presumably shared some of the benefits of the right hand working in the preliminary test, and made a better record than the right hand. Following the working of the right hand on the machine without work sheets, the left hand was the first to work in the semi-final test, working on a test comparatively new. The right hand then followed, benefited by the practice of the left hand in the semi-finals, and made a better record than any in the preliminary or semi-final. Such was not the order of working the hands with reactors Pi., and Pe., and there is no corresponding gain. The idle hand of the training series was worked last in the semi-finals, and made larger gains than the hand working first in the semi-finals.

TABLE 9

REACTORS	HOURS WORKED	PER CENT GAINED		ORDER OF WORKING HAND	
		By practiced hand	By idle hand	Preliminary	Semi-final
A.	9	R. 24	L. 6⅔	R. L.	L. R.
Pi.	10	R. 12	L. 17	L. R.	R. L.
Pe.	9	R. 13	L. 17	R. L.	R. L.

TABLE 10

Showing result of straight listing after "machine without sheets" work

REACTORS	HOURS WORKED	PER CENT GAINED		ORDER OF WORKING HAND	
		By practiced hand	By idle hand	Semi-final	Finals
A.	8	R. 17	L. 20	L. R.	R. L.
Pi	8	R. 18	L. 9	R. L.	R. L.
Pe.	9	R. 20	L. 22	R. L.	R. L.

Percentage calculated on records of preliminary tests.

After this work on the machine without work-sheets, the reactors continued at simple listing, the results of which are shown in table 10. The idle hand gained more than the practiced hand in reactors A., and Pe., and less than the practiced hand with reactor Pi.

Section F. Observation without training in number reading. One reactor, Wl. (Plate XXIII), after the regular preliminary tests, acted as observer to Pi., in the work without work sheets, and consequently did not receive the benefits of training in reading the numbers as did the others who acted as observers.[3] After the semi-finals, Wl., worked at straight listing. Table 11 gives a statement of the results.

Following the period of observation, Wl., worked at simple listing and adhered to a system of fingering which he had planned while watching Pi. work. At the very first, the subject experienced no little difficulty with this system, but held to it tenaciously, and this doubtless accounts in part for the small percentage of gain made by the left hand in the semi-final test. This same system of fingering was maintained by the right hand in all subsequent practice. The gain of 25 per cent made by the idle hand, following a practice period which netted a gain of 33 per cent to the hand which practiced, is about the same as the improvements made by the other reactors. The small percentage of gain (3 per cent) following observations without reading work sheets, compared with gains made by reactors listed in the semi-finals of section B, suggests the value of number-reading training.

TABLE 11

Showing results of work of reactor W. L.

METHOD	NUMBER OF HOURS	PER CENT GAINED		ORDER OF WORKING HAND	
		By right hand	By left hand	Preliminary	Semi-finals
Observation	10	14	3	R. L.	L. R.
				Semi-finals	Finals
Simple listing	9	33*	25†	L. R.	L. R.

* Working.
† Idle.

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[3] See Section B.

III. RÉSUMÉ AND DISCUSSION OF DATA

1. In every case of simple listing, there is improvement of the idle hand.

2. In every case of observation, there is improvement of both hands: and when followed by simple listing, there is improvement of the idle hand as well as of the practiced hand.

3. In every case of auditory listing, there is improvement of both hands, as shown by the semi-final test and specially of the hand which was idle in the auditory series. When followed by simple listing, the idle hand made improvements in the final tests.

4. The reactors reading the numbers without working the machine showed improvement in both hands in the semi-final tests, and in subsequent simple listing, the idle hand showed marked improvement in the final tests.

5. In the cases of reactors using the machine without work-sheets, the idle hand as well as the hand which worked showed improvement in the semi-final tests; and in the subsequent simple listing, both the practiced hand and the idle hand showed improvement in the final tests.

Every case, therefore, presents positive evidence of "bilateral transfer of training."

What is the explanation of this bilateral transfer of improvement? The following points may well be considered in answer to this question.

1. The emotional factor is probably responsible in part. The anxious, nervous attitude of the preliminary testing has given way to a definite "set" for record, and hence in the semi-final tests, better control of motor reaction is maintained.

2. General habits and conditions may have improved, e.g., more concentrated effort, greater ability to resist distracting factors, and less bodily fatigue.

3. Is the transfer in part due to the fact that a motor impulse from the "higher centers" leading to set reactions with one hand, has acquired influence on the other hand? (Wissler and Richardson (32), Davis (6)). The group practiced on the "machine

without work sheets" (section E) should give evidence on this point. Here the training was directly on hand manipulation, *plus* the learning of the keyboard. The average of percentages of improvement to the idle hands of the reactors in this series of training, evidenced at the semi-finals, was 13⅔ per cent. This group was trained on all the processes of simple listing except one, viz., the number-reading habit. They were directed to have in mind a definite number, and then put that number into the machine, and their statements are to the effect that the directions were followed. After the semi-final tests, simple listing was done by the hands which worked in the previous training series, resulting in improvement of 18 per cent as against 16⅓ per cent for the same hands in the semi-finals, but the idle hands in this last series made average gain of 16⅔ per cent, contrasting with the gain of 13⅔ per cent in the training series. This suggests that the training in the number reading habit leads to greater improvement with the idle hand than comes to the idle hand through practicing the other hand in working the machine, which is true also of the practiced hand.

4. Evidence that the influence of the "number reading" habit in improvement to the idle hand is even greater than hand training, is to be found in the record of the group who were trained in "number reading without machine" (section D). The average improvement of the reactors in this group, based on the records of the semi-finals, is for each hand (since neither hand was worked) 31 per cent R., and 20½ per cent L.: average of both hands of all subjects being 25¾ per cent. Following the semi-finals, the reactors worked at simple listing, R., nine hours, and Ir. seven hours. At the finals, these reactors showed average gains to the working hands of 14 per cent, to the idle hands 11½ per cent. Comparing these averages with those made by group E shows this fact: while in group E greater average improvement is made in the training course following the semi-finals, where number reading as well as hand manipulation was present, than was made in the training preliminary to the semi-finals where number reading was not present; in group D the greater average improvement was made at the semi-final following a training

course where number-reading was present in the training, though hand manipulation was absent, than was made in the finals, following a training course where both number reading and hand manipulation were present as elements in the task. The reactors trained in number reading without manipulation of the machine appear able to make better records when put to simple listing than reactors trained to manipulate the machine without number reading.

In considering the records of group B, observers who were so placed that they could read the work sheets and hence were trained in number reading and were able also to learn the keyboard and whose instructions were such that they would be led to identify the numbers of the work sheets with the digit buttons on the keyboard (section B), we find average of improvement at the semi-finals almost equal to the records of group D, viz., R., 23⅓ per cent, L., 25⅓ per cent, average of both hands of all subjects 24⅓ per cent.

The average gain of the practiced hands in the simple listing following the semi-finals was 18 per cent, while the idle hand during this simple listing made average improvement of 17 per cent.

5. The reactors who were given auditory training (section C) made at the semi-finals an average improvement to the idle hand of 31⅘ per cent; better percentage of improvement than that made by any other group, including those on simple listing. One reason that may be offered for this high percentage of improvement is the influence of the "urge" to speed due to the "pressure" of the reader. There is no time wasted between number listing in the auditory training and the habit of continuous working was apparent when change was made to simple listing at the semi-finals. Furthermore, while auditory training did not include visual perception of numbers or number reading, there was given training in number grouping and aural number perception and spatial location of number groups with reference to the keyboard. There was some increase in the time records at the semi-finals, and the simple listing following the semi-finals never reached as low a time record as that reached by auditory listing, but no subject trained in auditory listing noticed

any particular interference between the habit of auditory perception of numbers and visual perception of numbers, though all thought simple listing was slower than auditory listing.

IV. ERRORS

Each subject was told at the beginning of his work that he would be graded for speed and accuracy in evaluating his work. With one exception, however, each subject seemed to place more emphasis on the attainment of speed than on accuracy, and even when attention was called to the prevalence of errors, no apparent difference was noted.

There were nine kinds of errors made by the various subjects throughout the entire course of work.

a. Wrong digit, e.g., 5972 written for 6972.

b. Interchanging two digits, e.g., 3764 written for 3746.

c. Interchanging three digits, e.g., 3674 written for 3746.

d. Where wrong row of keyboard was used, e.g., 6606 written for 7707.

e. Omitting one digit.

f. Omitting an entire number.

g. Getting "out of bounds," i.e., not using the columns of digits prescribed for use but a column to the left or right of such prescribed columns.

h. Putting in some numbers not on the work sheet (occurred one time).

i. Repeating a number.

For drawing the error curve (on the sheet with the learning curve), it was advisable, if possible, to reduce these various errors to a common factor, and while the scheme used does not pretend to be perfect, it is the one appearing most available. All the errors listed above were reduced to terms of "a" excepting "f" and "i," which were held as equal and as of the same character.

In the scheme, then, *b* equals two *a*'s; *c* equals three *a*'s; *d* equals four *a*'s; *e* equals one *a*; *g* equals one *a*; *h* equals four *a*'s. The error curves are plotted accordingly, allowing one square for each *a*. Where *f*'s and *i*'s appear, the number are expressed in terms of *f*'s.

Causes of errors

The main cause of the inaccuracy in the work appears to be the desire to acquire and maintain speed. Hence, when the wrong digit was put in the machine, the power to inhibit the movement to press the print bar and to correct the mistake was frequently lacking, because of the compulsion of the "set for speed." Irrespective of speed, it was no difficult task to lose one's place on the work sheet, which accounts for the *f*'s and *i*'s. Why there should be any interchanging of digits, e.g., *b*'s and *c*'s, is a more complicated question. There is a tendency to group the digit pressing: e.g., 1928 is frequently written by pressing the digit of the thousands column 1, next the digit of the tens column 2, both at the bottom of the keyboard, and then 9 and 8 which are at the top of the keyboard. (This is the usual bank clerk's method.) In this manner of working the keyboard, may be found in part the reason for *b*'s and *c*'s. Failure to see the number as a unit group and reading it as a group of units without stressing the order of the units is also partly responsible for *b*'s and *c*'s.

Errors *d*, *e* and *g* were undoubtedly due largely to the impulse for speed, *d* and *g* being very rare and *e* occurring at times when the button for the proper digit had been pressed before the printing of the previous number was fully completed, and the button pressed by the reactor was released by the printing operation, without the knowledge of the reactor. This error is not really so chargeable to the reactor as it is due to the lack of speed of the machine. Accurate work was relatively rare.

In estimating the accuracy of the work of each sheet, the same scheme was followed as was used in plotting the error curves. Since each day had from four to six sheets, there are, as the largest number of grades of accuracy for each day, four to six according to the number of sheets used. If every sheet were accurate the minimum number of grades of accuracy would be one. (All the sheets were graded for relative accuracy by days and were compared with the speed grades of the same sheets.) The sheets of each day's work were graded for speed by the day.

It is obvious that the greatest number of speed grades would be six and the smallest number would be one for each day's work. The relative speed and accuracy grades are for each particular day's work and not intercomparable. Grade "1" in speed means that such a sheet was done as rapidly as, or more rapidly than, any other or all other sheets for that day, and grade "1" in accuracy means that such a sheet was as accurate as, or more nearly accurate than, any or all other sheets done that day. Accuracy grades, therefore, are relative for each day, and are not based on absolute accuracy as a standard.

TABLE 12

SPEED GRADES	RELATIVE ACCURACY					
	1	2	3	4	5	6
1	184	78	31	41	12	0
2	121	106	73	39	11	4
3	131	105	67	39	9	3
4	114	103	44	35	15	1
5	98	74	67	27	11	1
6	45	46	44	22	4	1

TABLE 13

Summary of speed and accuracy scores. Total of 312 days

SHEET	MOST ACCURATE	LEAST ACCURATE
Fastest	184	82
Slowest	103	75

Table 12 presents the totals, the columns marked "S" indicating the relative speed grades for the day's work, and columns marked "A" indicating the relative accuracy for the same sheets. From this table, it is apparent that 184 received first grade in accuracy and speed for their individual days: the highest score made in comparative speed and accuracy. The sheets having the highest speed grades, i.e., which were the slowest done, included also the smallest number graded accuracy first grade. That is to say, the slowest sheets included the smallest number receiving the most accurate grade.

Table 13 is based on the work of 312 days on which but one hand, either right or left, was worked, and the same amount of work was done by each hand with the exception that one day one subject was able to do only five instead of six sheets. While no sheet graded "1" in speed was graded "6" in accuracy according to the preceding table, this does not mean that at no time was the fastest sheet of the day the most inaccurate, since it was relatively rare to have six grades of accuracy in a day's work. Only ten times in a total of 1871 sheets, requiring 312 days, did this occur. But, while the fastest sheet was scored 184 times as having the highest grade of accuracy, it was scored 82 times as having the lowest score of accuracy, and the slowest sheet actually was the most accurate 103 times. The fastest sheets were most inaccurate 82 times and the slowest sheets most inaccurate 75 times. The apparent but not real discrepancy in the table is due to the fact that many times in the work of the various days there were ties in the accuracy and speed scores. Particularly was this true with reactor H., who cared more for accuracy than for speed, and made by far the least number of errors.

A comparison of the errors made in the succeeding days' work reveals this fact: increase of speed is accompanied with a slight improvement in accuracy in the work of thirteen reactors; five reactors tend to increase in inaccuracy as speed increases; the error curves of five reactors are fairly level. The percentage of improvement in accuracy in the thirteen cases cited above, however, is very much less than the percentage of improvement in speed.

If we take the error record of the first day's work of each reactor, and compare it with the error record of the last day's work of each reactor, the following facts appear: twelve reactors made fewer errors on their last day than on their first day; eleven reactors made more errors on that last day's work than on their first day's work. In the aggregate there were made on the first day's work 209 *a*'s and 23 *f*'s, and on the last day's work, 233 *a*'s and 72 *f*'s. The difficulty of keeping the proper place on the work sheet is made apparent in this increase in the errors marked *f*, nineteen out of twenty-three reactors having made this kind of error at least once on their last day of work.

It is clear that special effort to attain speed results in increased inaccuracy with eleven out of twenty-three reactors, working on the adding machine, and absolute accuracy was realized by none of the reactors. Though in the instructions, emphasis was laid as much upon accuracy as upon the speed, the increase in speed was much greater than the increase in accuracy. Wells (30) concludes that when attention is aimed at excessive speed in typewriting, more errors occur: and likewise, with extreme carefulness, errors increase: but there appears to be a general positive correlation between speed and accuracy.

Thorndike (24) finds that the slowest reactors working in addition make more errors than reactors who are most rapid. "The same individual may lose in precision by increasing his speed (though he will not always do so), but the sort of individual who is rapid, will tend to be accurate also." Woodworth (33), in relating speed to accuracy, thinks there is a lower limit beyond which decrease in speed does not conduce to accuracy in voluntary movements, and at the upper end, there is a limit beyond which increase in speed does not produce further increase in inaccuracy. The analysis of the data of this investigation agrees with these general conclusions.[4]

By averaging the time records and the error records of each reactor's work, and applying the Spearman method of rank, the correlation of grades of speed and accuracy is 0.1147 (18).

Optimal phase

1. Distribution of sheets according to speed in each day's work. In grouping the work on the basis of relative speed for the sheets of each day's work, the preliminary, semi-final and final tests were eliminated because on these days (a) only four sheets were used, and (b) both hands were practiced on the machine. Consequently, table 14 presents data of those days'

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[4] For absence of correlation of speed and accuracy, see also Thorndike and Woodworth (25).

work, with one hand, on which six sheets were worked, with the exception of E., who, on one day, worked only five sheets. Each sheet in each day's work was graded for speed for that day, according to the time in which it was done. Since there were six sheets for each day, there may be six speed grades for that day, or less than six speed grades, if the time required for any of the sheets tied. The fastest speed is graded "1," which means as fast as, or faster than, any other for that day. Succeeding digits represent grades of decreased speed.

The first sheet worked was the fastest the next to the least number of times. It was the slowest the greatest number of times. The last sheet was the fastest the greatest number of times, and the slowest the least number of times.

TABLE 14

SHEET	SPEED GRADES						TOTAL
	1	2	3	4	5	6	
1	52	51	50	63	72	62	350
2	61	58	78	74	56	23	350
3	44	60	91	74	55	26	350
4	72	75	74	44	52	33	350
5	72	94	58	54	49	23	350
6	97	65	63	59	45	20	349
Total in each speed grade...	398	403	414	368	329	187	

Because of ties, 62 in the sixth grade does not represent all of the first sheets that were actually the slowest of the day's run, for actually 110 times the first sheet took the longest time of 350 days; and for the same reason, 20 does not count all the "sixth" sheets that actually took the longest time of the day on which they were done, that number actually being 44. It is obvious that if two sheets of one day's work were done in the same time, they would tie for a speed grade. Such ties make the number of speed grades more or less than the number of sheets used.

If we group the sheets in respect to speed in two classes, the fastest and the slowest, and include in the fastest the sheets which received speed grades 1, 2, 3, and include in the slowest the sheets which received speed grades 4, 5, 6, the distribution of the sheets in these two speed grades totals as in table 15.

From this it is apparent that the tendency for greatest speed is found toward the end of the hour's work rather than at its beginning. This tendency was noted by Swift (22), who found that a "warming up process" was usually necessary, and consequently, the best scores as a rule were not the first scores of a day's series. Commonly, when the score was low enough to eliminate the effect of fatigue, the one or more high scores after the "warming up" period were followed by poorer scores, which again yielded to higher scores toward the end of the series. Book (3), in analyzing the drum records of typewriting practice, finds that for the middle or last part of the writing of almost every test, strokes come closer together on the records: while daily relearning and warming up were usually present, still, the first and last minutes were most productive.

TABLE 15

SHEET	SPEED GRADES						TOTAL
	1	2	3	4	5	6	
Fastest............	153	197	195	221	224	225	1215
Slowest............	197	153	154	129	126	125	884

2. Spurts. Marked variation in the speeds of operation of the machine in simple listing of separate sheets was apparent. Initial spurts, medial spurts and final spurts, occurred in the work of all reactors. Accurate account of these variations in speed was taken in the cases of the following reactors: E., F., Li., Mc., N., P. A time record in seconds, on a motor-driven kymograph, was made by the Johns Hopkins' Psychological Laboratory pendulum. The print-bar operations, marking the completion of the listing of one four-place number were registered on a kymograph's drum by an electro-magnetic marker. The electro magnet was in circuit with a simple contact attachment fastened to the under side of the print bar. When the print bar was pressed down to print the numbers, the circuit was

broken and the stylus made its record on the kymograph, on a line parallel to the line, and separated from it by about one-fourth of an inch. The work-unit in this part of the test was the work strip, i.e., forty-five four-place numbers (see page 322). It was thus simply a matter of counting the seconds required to list the first, second and last thirds of the work strip. Each day's work consisted of six work sheets, i.e., twelve work strips. Time was counted from the word "go" to the last print bar impression. In table 16, the figures at the head of the column represent the order of durations of the thirds of each strip, e.g., $1 < 2 < 3$ mean that the first third was the fastest third of the strip, and the last third was the slowest third; the other conventional signs are easily understood. The numbers in each column represent the number of times the rank order of thirds signified by the signs at the head of the columns occurred in the work of the various reactors.

TABLE 16

	E.	Fi.	Li.	Mc.	N.	P.	TOTAL
1 < 2 < 3	22	26	5	30	36	35	154
1 < 3 < 2	22	27	7	37	31	21	145
2 < 1 < 3	9	17	2	25	20	19	92
2 < 3 < 1	5	9	1	15	18	7	55
3 < 1 < 2	8	9	3	17	27	8	72
3 < 2 < 1	3	7	2	10	17	12	51
1 = 2 < 3	6	7	1	11	16	6	47
1 = 3 < 2	5	2	0	6	16	6	35
2 < 1 = 3	4	5	0	4	5	6	24
1 < 2 = 3	7	5	4	9	17	1	53
2 = 3 < 1	2	4	0	5	12	0	23
3 < 1 = 2	1	2	2	3	9	1	18
1 = 2 = 3	0	1	0	0	4	1	6
Total............	94	121	27	172	228	133	775

Table 16 shows that the first third of the work strip was faster than the second and third thirds 352 times out of 775 work strips, clearly indicating the presence of initial spurts. The second third is faster than the first and third thirds 171 times, and the third third is faster than the first and second thirds

141 times out of 775 work strips. The first third is faster than the second third 459 times, ties the second third 71 times, and is slower than the second third 245 times out of a total of 775 strips. The first is faster than the last third 491 times, ties the last third 65 times, and is slower than the last third 219 times out of 775 strips. The second third is faster than the last third 372 times, ties the last third 82 times, and is slower than the last third 321 times out of 775 strips. The evident conclusion is that the fastest speed is reached in the initial third of the work-units of the period, and the tendency to slow down persists through the entire work-unit, though in many cases, the medial and final spurts are present. This agrees with Wells (29) in his findings in the tapping test, and likewise with Chapman (4) in his investigation in addition, and with the results of the study on making hand-movements by Leuba and Hyde (13).

Comparing these results with the results of section 1 (*supra*), the following conclusions are warranted: 1. When the work of a period consists of an equal number of similar units, there is a tendency to perform the units toward the close of the period more rapidly than at the beginning of the period; 2. There is a tendency to perform the initial thirds of these individual units of the work more rapidly than the second and last thirds, the speed decreasing toward the close of the unit, though medial and final spurts are present.

The learning curve

Each large square on the base line of the curves signifies one day's work. The smaller squares indicate the separate sheets done on each day. The vertical line is the time line, each small square signifying five seconds. On the base line also the error curve is plotted according to the scheme detailed on page 351.

The learning curves are discontinuous, the breaks in the lines occurring when a change is made (a) from one hand to the other, or (b) from a special kind of training to simple listing. The letters "R" and "L" stand at the beginning of the curves for the right and left hands respectively.

One characteristic of all the curves is the rapidity of descent for the first day's work, irrespective of the order of working the hands. The improvement of the first day is frequently much more than the total improvement of all succeeding days. After the first day's work, the curve has a very gradual descent.

Considerable irregularity in the records of the individual sheets for the individual days is apparent. Since the time for the sheets was measured in minutes and seconds, relatively slight variations of absolute time, e.g., fifteen seconds, would make considerable deviation in the curve. The general trend of succeeding days is downward in spite of apparent irregularity of the curve of the individual days.

Evidence for "plateaus" is not very strong. In the cases of twelve reactors, very slight suggestions of leveling occur, lasting only three to four days, except with reactor N., when the last seven days' work was fairly on a time level. Reactor P., in the last two days' work, actually registered a loss in speed.

The practice period, in most cases being from nine to ten hours, was not long enough to reach a definite level of attainment which would require special effort to surpass. Reactors N., and P., whose practice included many more hours than the usual amount of work, did reach such levels.

Reactors at the beginning of a period occasionally reported headache, cold, etc., but such conditions did not seem to affect the speed records of that day. For example, H., did not sleep well the night preceding his eighteenth day, because of conditions in the boarding house, and had a headache on that day; but his work for that day did not seem to have been affected thereby when compared with the previous and following days.

GENERAL OBSERVATIONS

1. The reactors showed a noticeable degree of nervousness, exception Lo., who had had considerable experience in laboratory experiments prior to serving as a reactor in this experiment. This nervousness was greatly reduced by the end of the first day's work, and rarely increased thereafter. The change from

a training course to the semi-final test in sections B., C., D. and E. aroused to a small degree an element of nervousness which disappeared before the end of the day's work.

2. Each reactor began the work of simple listing by reading the numbers, with marked lip movements, some even audibly pronouncing the words. Lo. stopped this practice by the end of the first day's work, showing rare recurrences of the same. With the other reactors, it persisted to varying lengths, continuing throughout the work with reactors Pe., Ir., K., H. and Wl. Coincident with the change in lip movements in reading the numbers, there appeared the practice of reading the entire number at one glance. This habit was developed quite fully by all reactors except Mc., who regularly took the second glance to complete the reading of the numbers.

3. Each reactor, excepting Lo., used the forefinger to press the digit buttons on the first day of the work, Lo. using several fingers from the beginning. In every case except two, F. and H., the progress of the work was accompanied by learning to use two or more fingers, one reactor, C., using the thumb and all the fingers in a regular manner before the close of the practice period. F. and H. never used any finger other than the forefinger. The adoption of new fingering habits was concomitant with marked slowing of work in the cases of We. and W., in their semifinals, who had devised their own system of fingering while observing. Less noticeable results accompanied the habit of using more than one finger with the other reactors.

4. Reactors are not good judges of the speed of their work; frequently reactors would say that they felt they were not making as good speed on particular sheets as they had made on previous sheets of the day or the previous days. In most cases where such comments were made, the reactor was mistaken. Speculations about speed attainments were discouraged, but when they were made, they were almost invariably wrong, even though they seemed to be sincerely offered, and not for the purpose of learning about the speed record.

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A ROTATOR FOR VESTIBULAR AND ORGANIC STIMULATION

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This rotator was designed to carry a specially constructed chair seat for rotating human reactors in seated position, or a platform for human reactors in reclining positions, or smaller platforms for animals. The cut shows merely the rotator, without chair or platform.

A heavy cast iron frame, 20 inches high by 12 inches wide, mounted on a substantial cast iron base, supports a vertical mainshaft 1 inch in diameter, turning on heavy adjustable ball bearings, and carrying on the upper end a cast iron disc (*T*), 10 inches in diameter, to which chair seat and platforms are attachable by bolts. A worm wheel (*W*) with 30 teeth and a set of electro magnets (*C*), bolted together to form a single unit, are journaled on the vertical mainshaft, and revolve freely on it. A one-half horse power shunt-wound motor, 110 volts direct current, 1150 revolutions per minute, (*M*), mounted on the base, is belted to a cross-shaft journaled on the main frame, and carries on both ends flywheels (*F*, *F*). On the middle part of this cross-shaft is cut a worm, in mesh with the worm wheel (*W*).

The electro magnets (*C*) are wound on spools turned out of solid iron, and bolted solidly at their upper ends to an iron disc, centered in the vertical main shaft. There are six magnets, arranged at equal intervals around the disc. A similar set of magnets (*B*), is mounted below the first set, in an inverted position; i.e., the lower ends of the magnets being bolted to an iron disc. The disc carrying these magnets is fastened solidly to the main frame of the apparatus. Between the two sets of magnets is a planed iron disc keyed to the main shaft, having very slight play between the free ends of the two sets of magnets. Electric current connections are made with both sets of magnets, collecting rings being placed on the vertical shaft to lead in to the upper, rotating set.

Leads from the field and from the armature of the motor are brought out to binding posts, so that by connection with suitable rheostats and switches the motor can be reversed, and can be varied in speed both above and below its normal rating. At the normal motor speed, the vertical shaft is rotated at a little less than two seconds per turn.

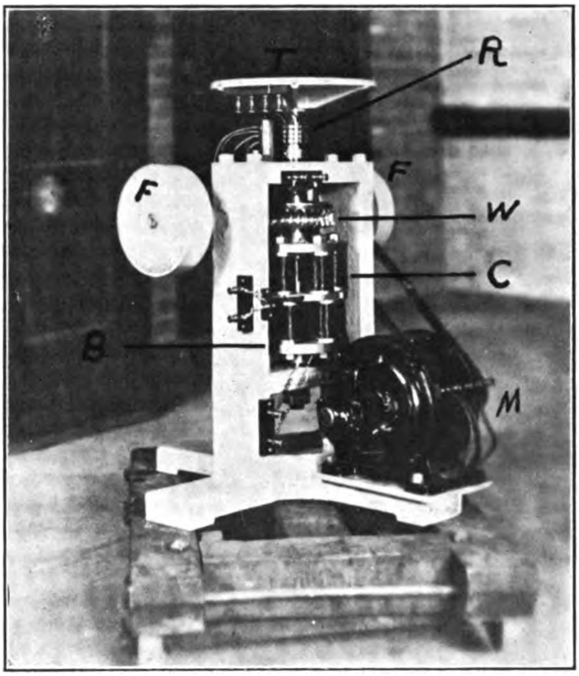

FIG. 1. ROTATOR WITHOUT CHAIR OR PLATFORM, MOUNTED ON A TRUCK FOR PHOTOGRAPHING

When the motor is started, the cross shaft is driven, and drives the worm wheel (*W*) and the gang of magnets (*C*) attached to it. When current is applied to the magnets (*C*), they "pick up" the iron disc armature keyed to the vertical shaft, and therefore set the shaft into rotation. This gang of magnets act as the clutch. This shaft, and the chair or platform bolted to the disc (*T*) attached to it, start with a "slip" determined by the load and the amperage of the current applied to the magnets. The acceleration is therefore controllable. When it is desired to stop the rotation of the vertical shaft, the current through the clutch magnets (*C*) is interrupted, and current is applied to the lower gang of magnets, which act as a brake, on the common armature disc. The retardation is controllable by controlling the current through the brake magnets. The motor, with the cross shaft, and clutch magnets continue in rotation.

Smooth and constant acceleration and retardation are obtained, and continuous rotation after full speed is obtained without shock or jar. The advantages of this method of stimulation as compared with the jerky and variable hand driven chair are enormous.

The apparatus is strong enough to sustain considerable strain. We have had no difficulty in rotating a man stretched out at full length on the table, even when three-fourths of his weight was on one side of the center of rotation. Without chair or platform, the apparatus weighs about 425 pounds.

In order to permit of electric connections with the chair or table when in rotation, for recording or stimulating purposes, two pairs of collecting rings (*R*) are mounted on the main shaft.

By means of a contact device, attached to the frame and operated by a series of elevations and depressions milled in the edge of the armature disc, which opens and closes an electric circuit ten times in each rotation, a kymographic record of speed, acceleration and deceleration can be obtained.

Two of these rotation apparatus have been built in the physics workshop of the Johns Hopkins University. Various necessary attachments for experimental purposes are being developed with the aid of Mr. M. W. Pullen of the Johns Hopkins University, and Dr. Percy W. Cobb of the Nela Research Laboratory.

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A NEW METHOD OF STUDYING THE IDEATIONAL BEHAVIOR OF MENTALLY DEFECTIVE AND DERANGED AS COMPARED WITH NORMAL INDIVIDUALS[1]

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In our various institutions for the treatment of mentally defective, deranged or criminalistic individuals, there is obvious and urgent need of methods which shall yield precise information concerning the ideational processes; for in large measure human conflict, inefficiency, and failure are due either to the lack of ideas or to their imperfect functioning. The comparative psychologist, interested as he necessarily is, in the genetic or developmental relations of ideational and other mental processes, seeks especially to devise methods which shall supply strictly comparable data concerning the characteristics and genetic relations of the various important mental processes and forms of behavior. It is one of his chief tasks to achieve more complete knowledge and a practical understanding of ideation through the study of its evolution in the race (phylogenesis) and of its development in the individual (ontogenesis). He views the psychological problems of psychiatry and psychopathology in the light of knowledge of mind and behavior which is generally considered irrelevant by the professional psychiatrist or psycho-pathologist. Yet here and there, much to the encouragement of those who believe firmly in the value of the comparative method and of the genetic descriptions of life which it yields, appear evidences that even those whose attention is monopolized by the practical problems of mental deficiency or disease are coming to see that no stage or condition of mental life or behavior can be understood or intelligently dealt with in the absence of thorough-going knowledge of its history and of its immediate relations to varied life-processes.

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[1] This manuscript was written in 1916.

Especially in the sphere of contact of psychology with the medical sciences, methods of research, as contrasted with practical tests, are sorely needed. Because of the difficulties of our practical tasks we are prone to aim rather too directly at useful results. Our tests are on the whole either too simple to yield reasonably accurate results, or they supply information too limited in its scope for safe interpretation and application. The method which I am about to describe is not a simple test, but instead a means of exploring certain of the ideational processes. It ultimately should yield results of practical value, but it will commend itself rather to those who wish a thorough understanding of reactive tendencies than to such as seek a rule of thumb for immediate use.

An experimental study of ideation may be (1) introspective, or (2) behavioristic, or (3) a combination of these two modes of observation. Since, while admitting the serviceableness of introspection, I am interested primarily in the study of behavior, the method which I have devised is behavioristic. But it should not be overlooked that it offers excellent opportunities for introspection whenever the subject is capable of such observation. I wish especially to emphasize the behavioristic character of the new method because physicians have too long ignored or neglected the scientific study of behavior, and this despite the fact that they might naturally be expected to take first place in the advance of our knowledge of human activities.

An appreciation of certain of the needs of information concerning ideational behavior having been forced upon me as psychologist in a hospital for the psychopathic I looked about for available methods of research. Only one promised to meet my requirements, and it, on close examination, proved to have so many shortcomings that I finally decided to use it as a basis for the development of a new procedure. The method to which I refer was devised by the psychiatrist and keen student of behavior, G. V. Hamilton.[2]

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[2] Hamilton, G. V.: A study of trial and error reactions in mammals. Jour. Animal Behavior, 1911, 1, 33–66.

Hamilton, in order to get knowledge of the reactive tendencies of normal and pathological, mature and immature human subjects in relation to those of infrahuman animals, has developed an apparatus and procedure at once ingenious and fruitful of practically and theoretically interesting and valuable results. He confronts his subject repeatedly with a certain situation. A compartment or room from which the subject seeks to escape is provided with four exit doors. In any given trial, only one of the four doors can be opened by the subject. The subject has no way of predicting which door will open. Only one of the four possible exists can be safely neglected, namely, the one which yielded escape in the immediately preceding trial, for *the same door is not left unlocked twice in succession.* As soon as a subject escapes through the appropriate exit, it is rewarded by the experimenter and after a certain interval returned to the compartment to try again to select the proper door. Thus, time after time, the behavior of a subject is observed in this definite and controlled situation.

The experimenter, as a result of repeated observations, discovers the various reactive tendencies of his subject and important characteristics of its ideational life. In the end his description of a given human or infrahuman subject's responses indicates the nature and degree of its ability to meet a situation which may be more or less adequately coped with according to the ideational capacity of the organism. Thus, certain persons, as well as certain lower animals, display the very ineffective method of trying the four doors at random, often the same one repeatedly. Yet other persons, or types of animal, exhibit the more satisfactory method of trying the doors in order from left to right, or from right to left, until the one which is unlocked is located. Still other subjects display a superior method in that they avoid the door which yielded egress in the previous trial and without repetition or waste of effort attack one, or if necessary all three, of the remaining doors.

Appreciation of the importance for the medical sciences, and especially for psychiatry, of Hamilton's method and of his results, the bulk of which are as yet unpublished, is just now beginning to develop; and it surely is not rash to predict that the kind of work which he inaugurated with mentally diseased and defective subjects more than ten years ago will shortly lead to important and far reaching improvements in the treatment of certain types of mental disorder. For after all it must be admitted, even by those who are most insistent on immediately and narrowly practical observational work, that the number, nature, and relations of our modes of adapting ourselves to the situations of life must be the chief diagnostic materials in case of many social and organic disturbances.

The method of Hamilton, which has been briefly described, presents a problem which is practically insoluble, yet whose solution may be approached in varying degrees, thus giving opportunity for the display of a great variety of reactive tendencies.

Taking Hamilton's procedure as my starting point, I devised an apparatus and technique which enable the experimenter to present to any subject—human or infrahuman, mature or immature, normal, defective, or diseased—problems *the solution of which gives opportunity for ideational reaction, although not necessarily depending upon it.* The problems which I finally decided to use may appropriately be called *relational* problems and the apparatus by means of which they are presented to a given subject I have rather inaptly named the *multiple-choice apparatus.*

The nature of a typical relational problem may be indicated by a simple example. Let us suppose that a group of like objects is placed before a subject. Some member of the group we shall define as the object to be selected or chosen by the subject, choice of it resulting in the obtaining of a reward, or the announcement of success. Let us further suppose that the right object, or the one to be chosen, may be adequately defined only in terms of its relation to the other members of the group. In case of a given problem, the relation might be *middleness.* Obviously, if the subject were told in advance, or by his own observation discovered, this definition, he should be able to select the right object immediately from any group which happened to be presented. To be sure, if the number of objects in the group became very great, the task of selection would be correspondingly difficult.

Now, as it happens, this relational method of studying ideational behavior has several points of merit for those comparative psychologists who seek to apply precise methods of studying behavior to the materials of the psychiatrist. These merits, it must be admitted, are not accidental, but instead result from the requirements which I had in mind in devising problems, apparatus, and procedure. The most important of them may be described thus: (1) A series of problems ranging in difficulty from the very simple and easy to the extremely difficult may be selected, standardized, and presented, either in part or in entirety, to any given subject. (2) Each of these problems is completely soluble by a subject with excellent ideational capacity, although not necessarily by a given subject. (3) The attempts of a subject to solve a problem are readily recordable as forms of reaction, for the most part as definite choices of objects in a group. The experimenter may, however, make time-measurements and keep, if he so desires, detailed records of behavior between choices. (4) Introspective data supplementry to those recorded under (3) may often be obtained from human subjects. Especially after success in the selection of the right object has been attained, it is important to request of the subject, if linguistic reaction is possible, a definition of the right object or a description of the method of selecting it. (5) Graphic representations of the process of problem solution, ideational or otherwise, are possible. Such, for example, are curves of learning constructed on the basis of right or wrong choices. Coefficients or indices of ideational capacity may also be used. Likewise, formula descriptive of the mode or modes of reaction, reactive tendencies or methods of choice become available.

APPARATUS

The original form of multiple-choice apparatus, with which at the Psychopathic Department of the Boston State Hospital the relational method was given preliminary trial, consists of a bank of twelve similar wooden keys before which the subject can be seated. Each key has electrical connection with a signal apparatus (bell or buzzer) which serves to indicate to the subject success in locating the right key. This simple and crude apparatus is shown in use in the accompanying figures 1 and 2. Any one or all of the twelve keys may be presented to the subject in a given trial. As the figures indicate, keys not in use are placed in a vertical position by the experimenter, whereas those from among which the subject is required to select the right one remain in a horizontal position and may be pressed by the subject in his effort to locate the one which will cause the signal apparatus to respond.

Before describing the procedure or further discussing the method, I shall describe briefly an improved form of multiple-choice apparatus which has now replaced the original.[3] The essential features of this improved model are: (1) A bank of twelve hard-wood keys which can be operated by the experimenter from one side of a screen and by the subject from the opposite side. (2) A signal and switch board on which are mounted twelve miniature lamps, each of which is connected with one of the keys. On the same switch board are mounted, also, twelve jacks, each of which is connected with one of the keys and may be connected with a small buzzer. (3) A wooden screen which serves to separate subject and experimenter and to prevent the former from seeing the movements of the latter and of thus being influenced in his selection of keys. (4) A box upon which the above parts are mounted and in which are housed the electrical mechanisms for the signal board.

This apparatus is shown fairly satisfactorily in figures 3, 4, and 5. Figure 3 is a rear view of the apparatus, showing the signal board with its twleve lamps and the jacks and plug by means of which any key may be connected with the buzzer. Beyond the signal board appear the metal clips, each numbered, with which the experimenter can either push out (expose to

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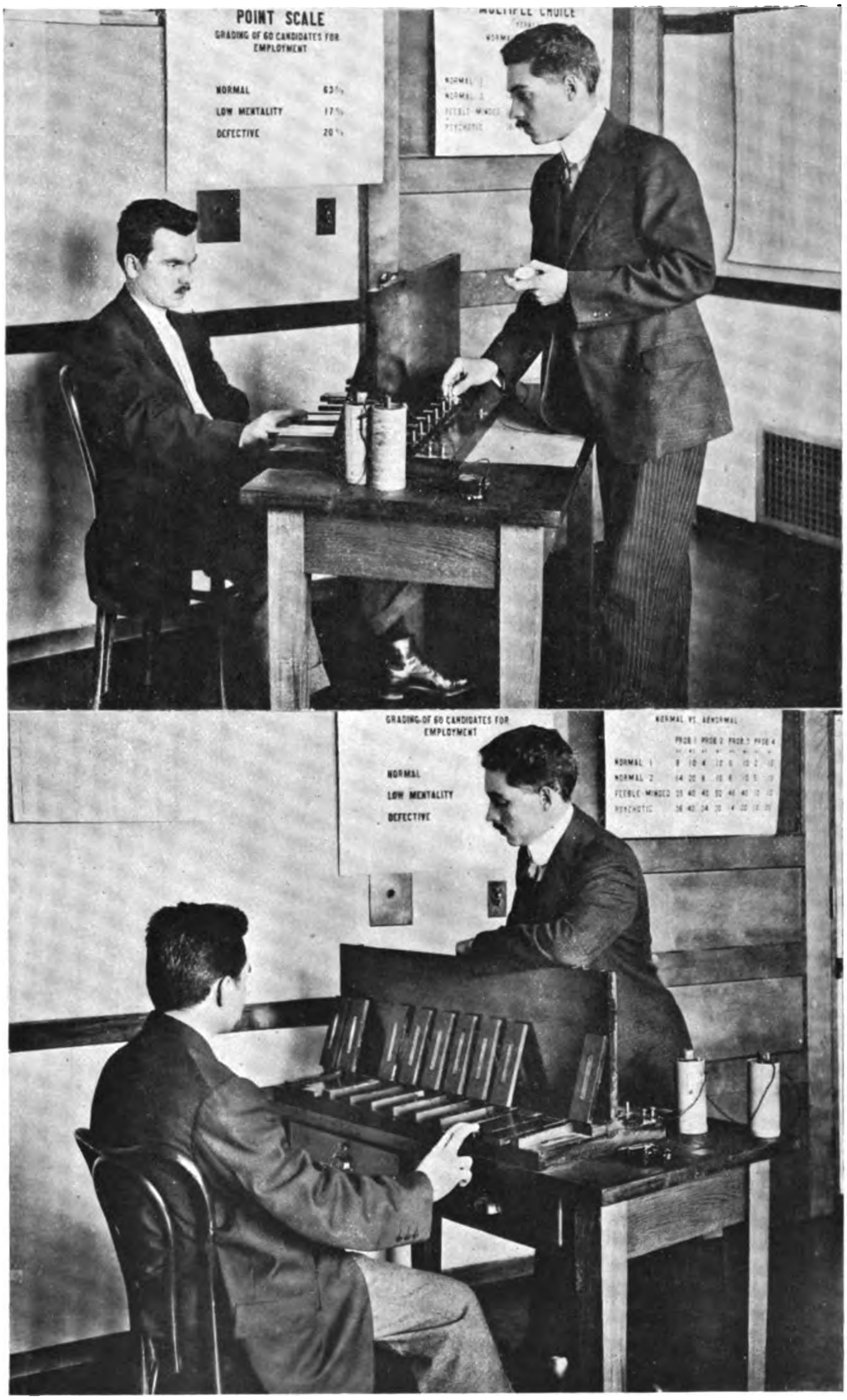
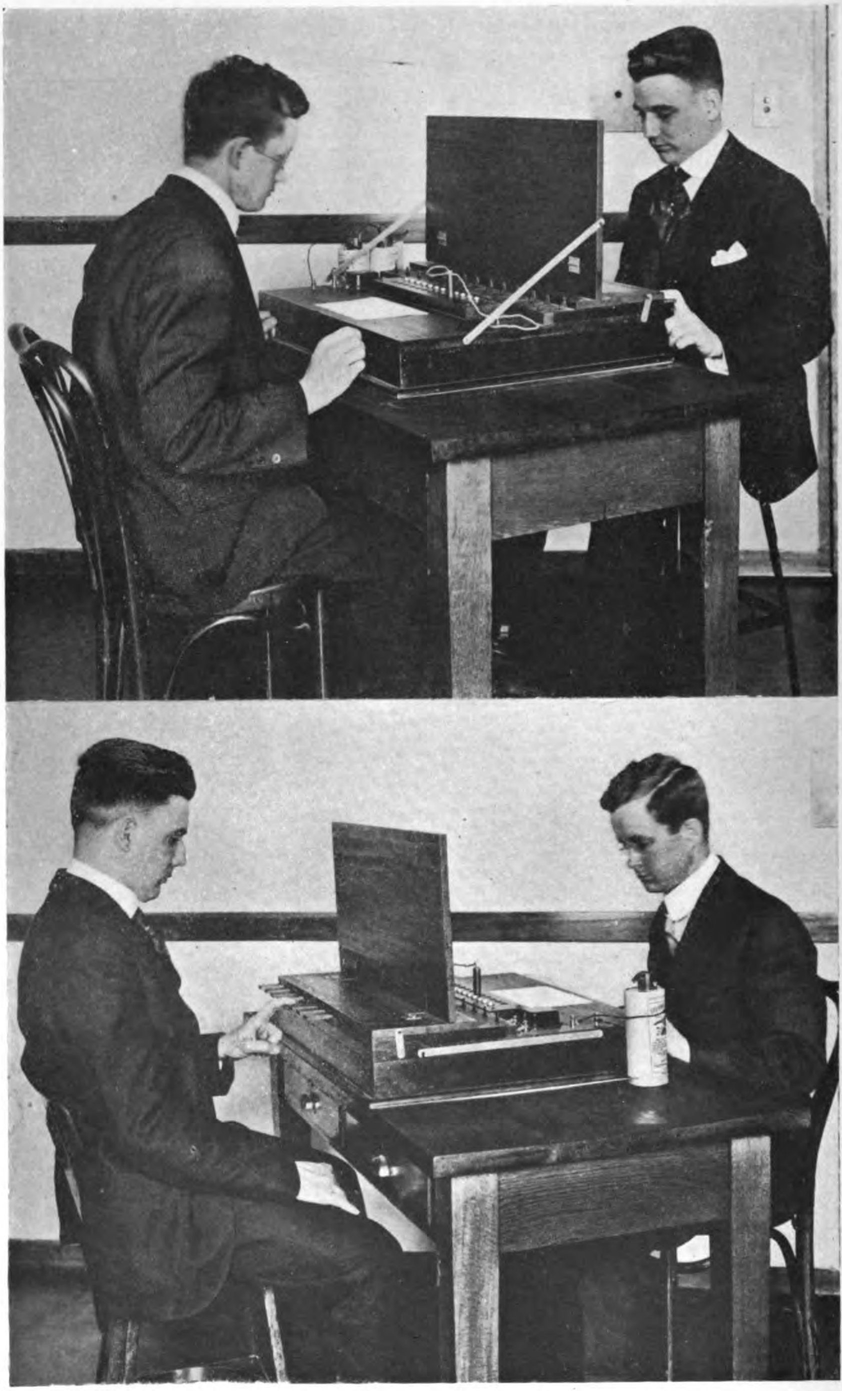
[3] This apparatus was constructed in the shop of the Harvard Psychological Laboratory.



Figs. 1 and 2. Figure 1, upper, and figure 2, lower; original multiple-choice apparatus, used in the Boston State Hospital, Psychopathic Department.



FIGS. 3 AND 4. Figure 3, upper, improved form of apparatus, rear view. Figure 4, lower, same, front view.

view) or withdraw a given key. A small metal-cased buzzer and two binding posts for connections with dry batteries appear at the left end of the box. As this figure shows, the screen separating subject from experimenter is hinged so that when the apparatus is not in use, it may be folded back over the signal board to protect it from dust or from any possible mishap.

Figure 4 is a view of the apparatus from the front, showing it as it appears to the subject. The keys are numbered (on the experimenter's side) from the right to the left, one to twelve. Keys nine to twelve are shown in their withdrawn position, that is, out of use or beyond the reach of the subject. Keys one to eight appear in position for use, that is, they project beyond their cover. The subject seated before the bank of keys is required merely to lightly depress the keys, one at a time. By so doing, he either makes a connection which causes the appropriate signal lamp for the key to flash, or in case the particular key happens to be the right one, it instead causes the buzzer to sound.

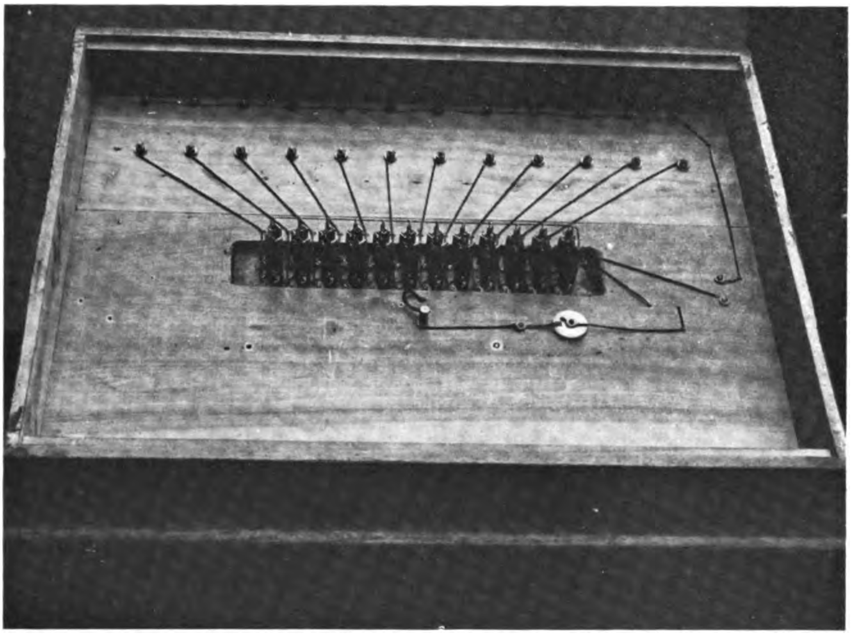


FIGURE 5. APPARATUS FROM BENEATH. SHOWING WIRING

Finally, figure 5 represents the apparatus as viewed from beneath, with the protecting floor removed. There appear in the foreground the row of jacks and a button resistance in the buzzer circuit. Beyond the jacks is a row of twelve lamp sockets, and the wires connecting these mechanisms with the several keys.

The apparatus, as thus designed and constructed, enables the experimenter to obtain accurate records of a subject's choices by following the flashes on the switch board.[4]

A brief statement should be offered with reference to the adaptation of the method for experimentation with infrahuman organisms and for the rapid testing of human subjects in connection with certain practical demands.

Multiple choice problems can be satisfactorily presented to a great variety of lower animals by means of a series of similar boxes, each of which is provided with an entrance and exit door. These boxes serve the same purpose as the bank of keys used for human subjects. The animal is required to select from a group of boxes whose entrance doors are open that one which bears a certain definite relation to the others, for example, the middle one. When this box is entered, the animal is rewarded with food, whereas when any other box is entered, instead of being rewarded with food, it is punished by being confined in that box for a certain interval and is then released and permitted to make another choice. This box type of multiple-choice apparatus has been used successfully with several birds and mammals.[5] It may, of course, be used for human subjects quite as well as the key-board form, but the presentation of the problems requires much more time.

Münsterberg suggested a convenient adaptation of the multiple-choice method for rapid mental examining. On small cards he had printed three lines of capital letters. Each line contains at least two different letters, for example A and O.

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[4] Electrical parts for the apparatus are supplied under the following numbers by the Western Electric Company: lamp no. 2-A, 4 volt; lamp cap no. 2-AY; lamp socket, no. 13; jack, no. 167; lamp cap tweezers, tool no. 112; lamp extractors, tool no. 116.

[5] See references at end of paper.

In case of some problems, the letters A, O, and U appear in a single line. The letters are so arranged that the O, for example, bears in each line a certain special relation to the other letters of the group, as for instance, the relation of second from the right end. It is the task of the subject to whom this "relation test" is presented to discover the relation of the O to the other letters on a given card. Different cards present relations of varying difficultness to discover and describe.

Yet another adaptation of the multiple-choice method has been made by the writer and the late C. S. Rossy for use in an adult point scale which they constructed.[6] In this case, circles and squares instead of letters are employed as symbols. A given card bears four lines of symbols, in each of which appear circles and one square. The relation of the latter to the circles is the same for each of the four lines. Experience indicates that the relational test has considerable practical value in mental examining as well as varied values as a method of research.

I wish, now, to describe as briefly as feasible the original multiple-choice procedure and certain features of the initial results obtained with normal and pathological subjects at the Psychopathic Hospital. The twelve-key apparatus described on page 373 was employed, and to each subject the following four problems were presented *First problem, first key at the left; second problem, alternately the first key at the left and the first key at the right; third problem, third key from the left end; fourth problem, middle key.*

Originally, the following explanation was made to the subject: "One key in the group will make a bell ring. Try to learn to find it. It will not be the same key twice in succession." The experimenter accompanied this statement by demonstration of the way in which the keys should be operated and of the fact that a certain key in a given group would cause a bell to sound, whereas the pressing of any other key had no such result.

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[6] Yerkes, Robert M., and Cecelio S. Rossy: A point scale for measuring the intelligence of adolescents and adults. Boston Medical and Surgical Jour., 1917, 176, 564–573.

It was shortly discovered that this form of explanation tended to complicate the experiment. Consequently, the following briefer statement was substituted: "One of the keys in each group will make the bell ring. Try to find it the first trial each time."

Even this statement proved far from satisfactory, and it seems probable that in the long run it may be better to demonstrate the mode of operating the apparatus and to offer no statement concerning the nature of the problem or what is expected of the subject.

For each problem a series of ten settings of the keys was arranged. In each of these a definite group of keys was presented. These so-called *settings* of the keys for the various problems appear in the accompanying reproduction of record sheets, pp. 381–382. The sheets indicate that in case of problem 1, setting 1, all twelve keys were presented to the subject, that in setting 2, keys 5 to 10 were presented, and so on. Following the data of columns 1 to 3 in the record sheets, which give, respectively, the number of the trial,[7] the keys in use, and the correct or signal key, appear the records of choices or reactions, that is, of keys pressed by the subject. Record sheet 1 presents the choices in each of the four problems for a normal man, while record sheet 2 similarly presents the results for a defective woman. The latter records, however, are not complete, since for problems 1, 2, 3, and 4 more than one series of ten trials was given. Comparison of record sheets 1 and 2 indicate that the normal subject solved the problems quickly and with few reactions and trials, whereas the defective subject required a much larger number of trials and made several times as many choices or reactions as did the normal man.

The experimental procedure always involved the systematic presentation of the different settings from 1 to 10 in order. A series having been completed, the experimenter unless for some reason the observations had to be discontinued, returned to setting 1 and once more presented the settings in order from 1 to

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[7] A given "setting" when presented to a subject is termed a "trial."

10. As a rule, not more than fifty *trials* were given a subject in succession, and in some instances even although success had not been achieved the work was abandoned after thirty trials.

Record sheet 1

Name: H. R. Age: 43. Nationality: American. Date: January 30, 1914.
Diagnosis: Superior normal.

PROBLEM 1				PROBLEM 2			
Setting	Keys in use	Signal key	REACTIONS	Setting	Keys in use	Signal key	REACTIONS
1	1—12	1	6. 8. 10. 12. 4. 2. **1**	1	1—12	1	5. 7. 9. 11. 3. **1**
2	5—10	5	6. 7. 9. **5**	2	5—8	8	**8** (Right key defined)
3	10—11	10	**10** (Right key defined after this trial)	3	3—10	3	**3**
	6—12	6	**6**	4	1—12	12	**12**
5	2—10	2	**2**	5	4—9	4	**4**
6	7—11	7	**7**	6	2—11	11	**11**
7	3—11	3	**3**	7	5—8	5	**5**
8	9—12	9	**9**	8	3—10	10	**10**
9	8—10	8	**8**	9	2—11	2	**2**
10	4—12	4	**4**	10	4—9	9	**9**

PROBLEM 3				PROBLEM 4			
Setting	Keys in use	Signal key	REACTIONS	Setting	Keys in use	Signal key	REACTIONS
1	1—8	3	4. 6. 8. 2. 7. 5. **3**	1	1—5	3	**3** (Definition anticipated)
2	10—12	12	**12** (Right key defined)	2	4—12	8	**8** (Definition formulated)
3	2—10	4	**4**	3	2—10	6	**6**
4	9—12	11	**11**	4	10—12	11	**11**
5	3—8	5	**5**	5	1—9	5	**5**
6	8—11	10	**10**	6	3—5	4	**4**
7	4—10	6	**6**	7	1—3	2	**2**
8	7—9	9	**9**	8	2—12	7	**7**
9	5—12	7	**7**	9	6—12	9	**9**
10	6—9	8	**8**	10	8—12	10	**10**

As soon as problem 1 had been solved, problem 2 was presented, with a definite statement that a change had been made from the first problem to a new one.

Record sheet 2

Name: B. T. Age: 18. Nationality: American. Date: May 12, 1914.
Diagnosis: Defective delinquent

PROBLEM 1

Setting	Keys in use	Signal key	REACTIONS
1	1—12	1	3. 4. 5. 8. 7. 6. 9. 10. **1**
2	5—10	5	7. 9. 10. 8. 8. 7. **5**
3	10—11	10	**10**
4	6—12	6	**6**
5	2—10	2	4. 7. 8. 6. 9. 3. 10. 4. **2**
6	7—11	7	**7**
7	3—11	3	6. **3**
8	9—12	9	12. 11. **9**
9	8—10	8	**8**
10	4—12	4	**4**
			etc.

No mistakes after twenty-sixth trial, but failed to formulate definition.

PROBLEM 2

Setting	Keys in use	Signal key	REACTIONS
1	1—12	1	8. 4. 7. 6. 2. 3. **1**
2	5—8	8	**8**
3	3—10	3	10. **3**
4	1—12	12	1. **12**
5	4—9	4	**4**
6	2—11	11	**11**
7	5—8	5	8. **5**
8	3—10	10	**10**
9	2—11	2	**2**
10	4—9	9	4. **9**
			etc.

No mistakes after nineteen trials. Definition formulated.

PROBLEM 3

Setting	Keys in use	Signal key	REACTIONS
1	1—8	3	4. **3**
2	10—12	12	11. 10. **12**
3	2—10	4	10. **4**
4	9—12	11	9. 12. 10. **11**
5	3—8	5	6. 4. 8. 7. 3. **5**
6	8—11	10	**10**
7	4—10	6	5. 7. 10. **6**
8	7—9	9	**9**
9	5—12	7	**7**
10	6—9	8	**8**

No mistakes after seven trials.
Definition formulated.

PROBLEM 4

Setting	Keys in use	Signal key	REACTIONS
1	1—5	3	4. 1. 5. **3**
2	4—12	8	6. 7. **8**
3	2—10	6	5. **6**
4	10—12	11	12. **11**
5	1—9	5	**5**
6	3—5	4	3. **4**
7	1—3	2	1. 3. **2**
8	2—12	7	6. 8. 2. 12. 4. 3. **7**
9	6—12	9	10. 11. 6. **9**
10	8—12	10	11. 9. **10**
			etc.

Failed to solve problem.
Given thirty trials.

A problem was considered solved as soon as a subject stated correctly the relation of the right key to the other members of the group, and *the number of trials preceding the first initially correct trial* after which no errors appeared was taken as a measure of the rapidity of solution. Either this datum or the total number of reactions may be used as the score. In case a subject failed to define the right key correctly, observation was continued until ten successive correct trials had been recorded, for occasionally it happens that a subject who is capable of fulfilling the demands of the situation by selecting the proper mechanism each time is quite incapable of formulating a description of method or a definition of the right key.

For the purpose of thoroughly trying out the method, of obtaining results for perfecting the technique and improving the apparatus, observations were made on approximately fifty normal subjects, all of whom were adults although varying widely with respect to education and professional training; on thirteen mental defectives, either adolescents or adults; and on somewhat more than fifty mentally deranged and diseased individuals, including cases of dementia praecox, manic depressive insanity, general paralysis, and drug psychoses. It is not the purpose of this report to present in detail the results of these observations, but instead merely to describe the method fully for the benefit of those who may wish to use it and to indicate certain general features of the results obtained with the groups of subjects referred to above. A complete account of the reaction of a subject to multiple-choice problems is given by the detailed records and supplementary notes made during experimentation, of which record sheets 1 and 2, pages 381 and 382 are offered as samples.

Obviously, it is not feasible to publish these detailed case records. Instead, it is necessary to find some way of summarizing the results. Analytic study of the data led to the following conclusions concerning feasible modes of treatment. Statistical data, although of considerable value, should by no means take the place of careful analysis and detailed description of individual reactions. Among the observational data which promise to be useful are (a) the number of trials necessary for the solution of a given problem; (b) the total number of reactions, that is, the number of keys operated in the several trials necessary for the solution of a problem; (c) the number of repetitions, that is, the number of times keys are used more than once in a given trial; (d) the number of ideas indicated by the objective records of response or by the introspection of the subject; (e) the method or methods of reaction, as for example, systematic use of the keys from the left to the right end of a group, or from right to left, or the regular trial of the even keys in order, or of the odd keys in order, random or haphazard procedure with repeated use of the same keys, etc.; (f) mental condition, that is, the general mental status or attitude in connection with reactions. It may obviously be one of confusion, of forgetfulness, or of definite insight, clearness, planning. (g) Types of reaction, such for instance as the tendency to begin at a certain point in each group or to favor a particular key or combination of keys. Such types of reaction are numerous and as yet insufficiently studied to warrant an attempt to list and evaluate them. (h) Formulation of definition. In varying degrees, subjects are capable of explaining their solution of a given problem. At one extreme are found individuals who although capable of responding correctly and thus of meeting the technical requirements for the solution of a problem, are yet utterly incapable of describing their method or even of stating the relational problem which they have solved *in motor terms*. Individuals incapable of defining the right key appear among normal as well as among defective and pathological subjects.

In multiple-choice experiments it is obviously essential that the observer keep complete records not only of the subject's reactions but also of his verbal comments or incidental bits of introspection and of such introspection as is elicited by questioning. As soon as a record has been completed, the results should be carefully analyzed.

There are presented in table 1 certain statistical data for forty-eight normal subjects, grouped in four categories. The primary classification is according to sex, but each sex group is divided into individuals of superior native ability and educational opportunity as contrasted with those of ordinary to poor native ability and education. For each of the four groups the average score, number of reactions and number of repetitions is presented for the problems described on page 379, and numbered in the table, 1 to 4.

TABLE 1

Summary of results for normal subjects

	TRIALS	REACTIONS	REPETITIONS
Problem 1:			
Superior males	2.62	9.24	0.05
Medium males	8.90	26.80	1.10
Superior females	2.11	9.22	0
Medium females	9.50	30.75	1.25
Problem 2:			
Superior males	3.33	12.76	0
Medium males	4.40	13.90	1.30
Superior females	4.67	22.11	0.78
Medium females	8.62	28.50	3.37
Problem 3:			
Superior males	1.19	4.81	0
Medium males	3.00	10.50	0.50
Superior females	2.44	9.89	0
Medium females	9.12	30.00	0.37
Problem 4:			
Superior males	3.05	10.62	0.14
Medium males	5.80	18.40	0.90
Superior females	2.89	9.78	0.11
Medium females	12.12	37.37	1.50

Particularly striking, in this table, are the numerical differences for the subdivisions of the sex groups. Unquestionably, education has much to do with the facility in the use of ideas exhibited by the superior individuals. Yet is it probable that native ability also has much to do with the contrasted results of the several groups. It should also be noted that the frequency of repetition is very much greater for the males and females of medium ability than for those of superior ability.

It may be remarked that the responses of a subject to a series of multiple-choice problems, no matter how the results be analyzed later or what significance be attached to them in the light of statistical data are surprisingly illuminating to the observer, for they indicate in a remarkable manner the ideational characteristics and efficiency of the subject. It seems to the writer fairly clear that some such procedure as this relational method may profitably be adapted to the needs of the mental examiner as a means of measuring for practical purposes such ideational characteristics of human subjects as number of ideas, their quickness of development and value.

TABLE 2

Summary of results for mentally defective subjects

	TRIALS	REACTIONS	REPETITIONS
Problem 1	9.9	26.0	0.4
Problem 2	17.0	35.5	0.5
Problem 3	5.6	16.4	0.1
Problem 4	23.7	73.7	4.2

By contrast with the results for normal subjects, there are presented in table 2 comparable data for thirteen mentally defective individuals, who, however, do not constitute a homogeneous group, since they exhibited varying degrees as well as kinds of mental deficiency. It will be observed that, in general, the figures for the group are larger than those for any one of the four groups of normal subjects and that problem 4, which for the normal individuals proved on the whole slightly more difficult than the others, required a large number of trials and reactions involving many repetitions in case of the defectives. Intensive study of such differences is sure to prove fruitful of results valuable to genetic psychology and also to psycho-pathology.

Especially promising of valuable results, as indicated by the data thus far accumulated, are comparisons of the reactions of normal children, ranging in age from three years onward, with those of various sorts and degrees of mentally defective individual.

It has not seemed worth while to present statistical data for the group or groups of mentally deranged cases observed. Averages, it appears, are rather more likely to conceal than to reveal the significant facts. The group is almost hopelessly heterogeneous, for in addition to individual, age, race, sex, educational, and social psychological differences, there appear the induced differences of mental disease. Moreover, even when an attempt is made to lessen this heterogeneity by classifying the subjects according to diagnosis into such groups as dementia praecox, general paralysis, and so on, the degree of heterogeneity is still extremely great because the various cases were studied in different stages of the development of their diseases. Chiefly for this reason it has been decided to present four reasonably typical cases instead of statistical data. These cases are hereafter designated by initials as follows: A. C., female, aged twenty-four, dementia praecox (hebephrenic); S. W., female, aged forty-eight, manic depressive (depression); S. S., male, aged thirty-six, general paralysis (with insight); C. C., male, aged fifty-one, Korsakoff's psychosis.

For each of these subjects there are presented in table 3 the detailed records of reaction to problem 2, in which the right key is definable as alternately the first key at the left end of a group and the first at the right end of a group. At the head of the table are indicated the ten successive settings of keys, S.1 to S.10. The keys presented and the right key appear under the number of the setting. Thus in S.1 all twelve (1–12) were presented and the first key (1) was the right one. The first vertical column of the left presents the number of trials in groups of ten. For each trial the numbers of the keys depressed appear in the several vertical columns. Subject A. C., for example, in his tenth trial (setting 10) depressed in order keys 4. 5. 6. 7. 8. 9. In his fiftieth trial (also setting 10) he depressed key 9 immediately.

The records of response speak clearly enough for themselves. The dementia praecox patient A. C., thought aloud,

TABLE 3

Results for mentally diseased cases

Problem 2

TRIALS	S. 1 1-12 (1)	S. 2 5-8 (8)	S. 3 3-10 (3)	S. 4 1-12 (12)	S. 5 4-9 (4)	S. 6 2-11 (11)	S. 7 5-8 (5)	S. 8 3-10 (10)	S. 9 2-11 (2)	S. 10 4-9 (9)
A. C., Dementia praecox (Hebephrenic). Female, age twenty-four										
1—10	1	5. 6. 7. 8	10. 3	1. 5. 11. 3. 2. 4. 5. 6. 7. 8. 9. 10. 11. 12	4	2. 11	5	10	2	4. 5. 6. 7. 8. 9
11—20	2. 1	5. 8	3	1. 12	9. 4	9. 11	8. 5	1. 3. 10	11. 2	4. 9
21—30	12. 1	8	3	11. 12	4	2. 11	5	10	2	9
31—40	12. 1	5. 8	10. 3	12	4	2. 11	8. 5	10	11. 2	4. 9
41—50	12. 1	5. 8	3	12	9. 4	11	7. 5	10	11. 2	9
S. W., Manic depressive (Depression). Male, age forty-eight										
1—10	2. 7. 5. 6. 4. 9. 8. 10. 11. 5. 2. 3. 4. 5. 1	5. 7. 6. 8	4. 3	2. 4. 3. 7. 6. 5. 1. 9. 10. 12	4	3. 2. 9. 11	5	9. 3. 4. 7. 9. 10	3. 2	9
11—20	5. 4. 1	5. 8	3	9. 6. 2. 4. 5. 1. 10. 12	4	4. 2. 7. 11	5	3. 4. 6. 10	10. 2	4. 9
21—30	1	5. 8	4. 3	3. 1. 2. 5. 12	9. 7. 4	2. 4. 11	5	3. 10	2	9

S. S., General Paralysis. Male, age thirty-six

1—10	2. 3. 4. 5. 6. 7. 8. 9. 10. 11. 12	5. 6. 7. 8	10. 9. 8. 7. 6. 5. 4. 3	12						

C. C., Korsakoff's psychosis. Male, age fifty-one

1—10	1	5. 8	3	12	6. 4	6. 5. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11	5	3. 10	11. 2	7. 4. 6. 5. 8. 9
11—20	1	5. 8	5. 7. 6. 3	12	4	9. 2. 4. 3. 5. 6. 7. 8. 9. 10. 11	8. 7. 5	10	2	7. 5. 6. 4. 8. 9
21—30	1	7. 5. 8	3	12	6. 9. 4	11	7. 8. 5	7. 6. 4. 3. 8. 9. 10	2	9
31—40	1	5. 8	8. 10. 3	1. 12	7. 6. 4	2. 3. 10. 11	7. 6. 5	7. 6. 5. 4. 3. 8. 9. 10	10. 11. 2	9. 4
41—50	12. 1	7. 5. 6. 8	10. 3	12	4	11	7. 6. 5. 8	5. 3. 4. 6. 7. 8. 9. 10	2	9

saying "It's the first one," "It's the last one," "It's the first one again." In trial 8, she exhibited an interesting ideational response by pressing the end keys 3 and 10, simultaneously. At various times in the continuous series of fifty trials given to this subject, she seemed on the verge of solution of the problem. Thus, for example, after trial 6 she reacted correctly three times in succession and asked, shortly, why the end keys were used. But in subsequent trials, her responses clearly indicated inability to grasp the principle of alternation, and finally after fifty unsuccessful trials, the method was explained to her in order that the next problem might be presented. She nearly succeeded in solving problem 1; failed on problems 2, 3, and 4. In her work a slight improvement appeared because of memory for the particular settings. She, however, exhibited little insight, talked freely and often foolishly, was deluded, and gave evidence of inferior mentality, apart, perhaps from the deteriorating effects of her mental disease.

S. W., the subject of depressive insanity, worked without insight and in ways extremely difficult to predict or to account for in ideational terms. He failed to solve, within the number of trials allowed, any of the four problems. At the end of his twentieth trial with problem 2, he remarked "They change all the time. I don't see how anybody could tell," and still later he volunteered "It's chance." In the three series of trials given to him in problem 2, he showed marked improvement, especially in the direction of lessening the number of reactions per trial. This is precisely what appears in low grade defectives among human subjects and in such animals as the monkey, pig, and cat. A careful comparative study of the results for various types of animal, grades of mental deficiency, and conditions of mental deterioration should prove quite as illuminating to the psycho-pathologist as to the genetic psychologist.

S. S., the general paralytic, worked on a wholly different plane from that of the other subjects, for he possessed insight, and although annoyed by the demand for intellectual effort, as a favor to the experimenter he worked faithfully at the multiple-choice problems. Problem 1, he solved immediately, grasping the idea of first at the left during the first trial. Problem 2 required three trials, in each of which, as the data of table 3 indicate, ideas are evident. Thus, for instance, he avoided key 1 in his first trial because it had been the correct key in problem 1. He then systematically tried each key in order from 2 to 12, and having eliminated the others, pressed key 1. Similarly, in the second trial, he worked systematically from left to right, through the group of keys. In the third trial, pursuant of the idea suggested by success with the last key in trial 2, he pressed key 10 first, and thence worked systematically toward the left, until he achieved success. In the fourth trial, he reacted at once correctly on the basis of the idea of first at the right end, and previous to his reaction he formulated the definition "First extreme left, then extreme right." The third problem he solved immediately, formulating the definition after the first trial and putting it to the test in the second trial. The fourth problem baffled him. Failure irritated and discouraged him. With urgence he continued to work through thirty-seven trials. He formulated the definition "Middle key of all keys down" after thirty-five trials.

It is peculiarly interesting and puzzling that a subject with the insight and ideational capacity of S. S. should have had so much difficulty with problem 4 after solving the other three problems easily. This result is sharply at variance with the data yielded by normal subjects, for even children of six to ten years have, so far as data are available, as a rule solved problem 4 quickly. It is natural to suppose that fatigue may have had much to do with the general paralytic's difficulties with this problem. His records indicate that he tried out a considerable number of complex relational hypotheses.

C. C. exhibited no enthusiasm for the experiment. He worked patiently, and after fifteen trials solved the first problem. In the second problem, fifty trials failed to give indication of approaching solution. He mostly tried the end keys, but he was not consistent in this, and the results indicate that he failed to discover that only the end keys yield success. The results, unlike those for S. W., show no lessening of the number of reactions with successive trials. In this respect they are in harmony with those obtained from other cases of alcoholic psychoses. Almost without exception such patients have extreme difficulty with ideational tasks. Their ideational efficiency is apparently greatly impaired by the deteriorating effects of alcohol.

TABLE 4

Suggested problems to be used with multiple-choice apparatus and method for testing human or other subjects

Problem 1. The same key in each trial (key or mechanism No. 5 from the left end of apparatus).
1–6 (5); 3–12 (5); 4–6 (5); 5–9 (5); 2–10 (5); 4–5 (5); 4–10 (5); 3–6 (5); 1–8 (5); 5–10 (5).

Problem 2. The first mechanism at the left end of a group.
6–12 (6); 11–12 (11); 3–11 (3); 1–5 (1); 4–11 (4); 10–12 (10); 5–9 (5); 2–12 (2); 8–11 (8); 7–12 (7).

Problem 3. The middle mechanism in a group.
1–7 (4); 10–12 (11); 6–10 (8); 1–11 (6); 1–3 (2); 4–10 (7); 1–9 (5); 9–11 (10); 1–5 (3); 6–12 (9).

Problem 4. The third mechanism from the right end in a group.
1–6 (4); 5–8 (6); 3–12 (10); 1–3 (1); 7–11 (9); 2–10 (8); 1–7 (5); 2–4 (2); 2–9 (7); 1–5 (3).

Problem 5. Alternately, the first mechanism at the left end and the first mechanism at the right end of groups.
8–12 (8); 1–10 (10); 3–8 (3); 6–9 (9); 1–9 (1); 3–5 (5); 7–11 (7); 5–12 (12); 2–8 (2); 4–6 (6).

Problem 6. The next mechanism at the left each time, beginning with mechanism No. 12, that is, progressively from the right toward the left end of the apparatus.
10–12 (12); 6–12 (11); 3–10 (10); 8–12 (9); 8–10 (8); 1–9 (7); 5–8 (6); 4–9 (5); 2–11 (4); 3–7 (3).

Problem 7. The mechanism to the left of the middle mechanism of a group.
6–12 (8); 3–5 (3); 8–12 (9); 1–9 (4); 2–12 (6); 10–12 (10); 5–11 (7); 1–5 (2); 3–9 (5); 1–3 (1).

Problem 8. Alternately, the second mechanism from the right end of a group and the second mechanism from the left end of a group.
6–12 (11); 2–5 (3); 1–8 (7); 5–9 (6); 1–5 (4); 4–12 (5); 5–10 (9); 9–11 (10); 2–9 (8); 1–5 (2).

Problem 9. The mechanism to the right of the mid-point in an even group, or the mechanism at the right end of the first half of a group consisting of an even number of mechanisms.
3–10 (7); 1–4 (3); 2–7 (5); 1–2 (2); 3–12 (8); 8–11 (10); 5–12 (9); 1–10 (6); 1–6 (4); 11–12 (12).

Problem 10. Alternately, the mechanism at the left of the middle mechanism and the mechanism at the right of the middle mechanism.
1–7 (3); 8–12 (11); 2–10 (5); 10–12 (12); 1–9 (4); 3–9 (7); 1–3 (1); 6–10 (9); 6-12 (8); 3–7 (6).

SUGGESTIONS FOR THE FURTHER USE OF THE MULTIPLE-CHOICE METHOD WITH HUMAN SUBJECTS

It is recommended that the improved form of the key-board apparatus be used for human subjects except where comparison of results with those obtained with lower animals is contemplated. In the latter case, it seems advisable to use some form of box mechanism for human as well as infrahuman subjects. Such mechanisms have been described by the writer in various reports of results.[8]

It is further recommended that the ten relational problems of table 4 be thoroughly tried out with a view to evaluation, revision or substitution, standardization and arrangement in servicable order. Following the designation of each problem by definition of the right key is a series of ten settings. The total number of keys used in each of these series is 60. This list of problems with the settings suggested, was originally published in the Journal of Animal Behavior, 1917, 7, pp. 27, 28. Discrepancy will be noted between the series as published here and in the Journal of Animal Behavior, as follows: In the case of problem 4, setting 8 was originally mis-printed as 3-5 (2). It should have read 2–4 (2). In the case of problem 9, setting 5 was originally printed 3–12 (11). It should have been 3–12 (8). Setting 9 was originally printed 5–10 (8). It should have been 1–6 (4).

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[8] See especially Jour. Animal Behavior, 1917, 7, 11–28, and Behavior Monographs, 1916, 3, No. 1, IV+145 Pp.

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THE EVOLUTION OF NERVE MUSCLE MECHANISMS

S. BENT RUSSELL

Among men of learning it is admitted that all forms of life that we find today, are the result of evolution. All higher forms were evolved from some lower forms. The earlier forms of animal life must have descended from forms akin to those we find in plant life. If we were seeking for the most interesting stages in the evolution of vertebrate animals, we would without doubt include among them the development of nerve-muscle mechanisms. The first contractile fiber in a living creature marked a most important step in evolution. The first nerve fiber also marked a step of the greatest importance.

To understand human behavior, we must study the behavior of all creatures having nerves and contractile fibers such as are found in men and horses. Every one knows that men and horses do work with their muscles. Every one knows that by cutting certain nerves in a man or a horse, certain muscles will be paralyzed and so put out of commission. Anatomists tell us that the muscles of men and horses contain contractile fibers and that similar fibers are found in worms and in creatures of still lower organization. Nerve fibers found in men and horses are of the same type as those found in worms and creatures of still lower organization. Neither muscle fibers nor nerve fibers, however, are to be found in plants. It is true that there are mechanisms in plants that are akin to sense organs. There are also mechanisms that produce movement, usually in connection with growth. Modern works on physiology contain a considerable fund of knowledge about nerve fibers and muscle fibers. Certain facts about their structure are revealed by the microscope. Their chemical composition is known to some extent. Certain electrical phenomenon have been observed. In the study of these matters there is still, however, much room for controversy.

There are many men of learning who have reached the conclusion that the behavior of men and animals can be explained by the interaction of chemical, electrical and physical forces or in other words, by the operation of mechanisms. They find that what a man does, what he says and also what he thinks can be explained by the operation of nerve-muscle mechanisms.

Now if you agree to these conclusions, one problem that will present itself to you is, how shall we explain the evolution of nerve muscle mechanisms? We shall seek to solve this problem in some measure in this discussion.

As our first step, we will note that respiration with oxidation is characteristic of animal life. An animal must have three things, viz.: *Food* that consists of organic matter; *air* for respiration with oxidation; *stimuli* from the environment.

Food is assimilated, oxygen is absorbed. As the organic matter is combustible or oxidizable, an explosive or unstable equilibrium condition is thus in some way produced. A stimulus from the environment upsets the equilibrium and the oxygen combines with the carbon and hydrogen and energy is released.[1]

One question that presents itself is, in the earliest stage of evolution, was the release of energy continuous or intermittent? It may have been continuous originally and by gradual steps grew to be intermittent. The continuous type of release may be illustrated by a gas burner. Here we have a continuous oxidation with release of energy. The same thing may occur in organisms although it must be, of course, at a moderate temperature. In most cases, we may suppose, the energy released would appear as heat. It might, however, appear as light or electricity.

If the intermittent release of energy is to prevail in the organism, cycles must be established as in a gas engine. In a gas engine, the cycle is made up of four stages, viz.:

1. The air and gas flow into a chamber where they mix.
2. The flow is interrupted.
3. An electric spark excites an explosion.
4. The products of combustion flow out of the chamber.

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[1] Arthur Dendy, Outlines of Evolutionary Biology. New York, 1913, p. 7.

In the animal, we may have a similar case. The four stages are:

1. Oxygen bearing matter is brought into the same space with carbon bearing matter forming an unstable mixture.
2. The flow ceases.
3. Some change in the environment produces a disturbance that excites a chemical combination. The oxygen combines with the carbon bearing compounds and energy is released.
4. The oxidized products of combustion are removed from the place concerned and the cycle is complete.

To proceed, let us think of a primitive organism in contact on one side with a mass of food and on the other side with water containing free oxygen. We will say the organism is made up of zones, an inner zone in contact with food and an outer zone in contact with water and oxygen. Between these two zones will be an intermediate one which will contain at times a mixture of combustible matter from the food and oxygen bearing matter from the outer zone. If conditions were favorable, there would be continuous combustion in the intermediate zone and a continuous release of energy. We will suppose, however, that there are such variations in the supply of food and of oxygen, that there will be periods of combustion and energy release alternating with periods of rest. After further development, we will suppose that there are periods in which oxygen bearing matter is partly mixed with combustible matter but not combined chemically. The oxidation after one of these periods will be more rapid. There will be, therefore, a greater release of energy. After the oxidation period, there will be a period during which the oxidized products are passing away.

To justify the assumptions we have made regarding combustion, let us note that it is quite possible on one hand to have such a meeting of carbon bearing compounds with oxygen bearing compounds that a continuous combustion will take place without the aid of heat of great intensity. It is quite possible on the other hand, to have a mixture of carbon bearing compounds with oxygen bearing compounds such that combustion will not take place without an igniting impulse of some kind. By way of illustrating this situation, take as an example, a mixture of air and gasoline vapor. There will be no combustion at moderate temperature. An electric spark, however, will ignite the mixture and a rapid combustion will occur. The resulting explosive action may be viewed as a response to the stimulus of the spark. We assume that likewise, there can be a mixture that will not react until there is an exciting impulse and that this impulse is of a different character from an electric spark but sufficient to provoke combustion in such a mixture.

We see then that in the intermediate zone by degrees, a development will be reached where there will be at times oxidation of an explosive nature with release of energy followed by an escape of the products formed. Thus we have the beginning of movement.

Let us say that a part of the outer zone is concerned in the movement. This part will naturally be located where it is most exposed to changes in light or heat. Let us note here, that when a movement takes place in part of an organism, the other parts will be more or less affected by it. We will assume then that the movement causes changes in the adjacent passive parts that serve to facilitate later movements. For example each movement may promote the oxidation processes by bringing more oxygen within reach of the organism. In some such way each movement may come to prepare the way for a succeeding movement.

Now, the release of energy that we are considering, while it is of an explosive nature is of course, not the same as an explosion of gunpowder or of gasoline vapor. It is a rapid rearrangement of atoms in the molecular pattern. We do not know just what the changes are, but we know that movement takes place and without much heat. It is not, therefore, combustion in the ordinary sense. To avoid misunderstanding, let us hereafter use the term "eruption" to express a chemical reaction with release of energy. There is reason to think that these reactions are of an electrical nature; in some cases at least.[2] We will therefore agree that the term eruption covers electrical as well as purely chemical reactions. Let us also use the term "surcharged mass" to express a mixture that is explosive or unstable, i.e., it contains both oxygen and combustible matter in proper proportion so that a certain kind of shock will produce an eruption.

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[2] Wm. M. Bayliss, Principles of General Physiology. London and New York, 1920, p. 183.

Let us now suppose that the intermediate zone contains a surcharged mass. Presently comes a disturbance from the environment which excites a reaction at the nearest point of the mass. The reaction beginning at this point is communicated through the entire mass. The eruption spreads from point to point like the burning of a powder train until the entire surcharged mass is mobilized.

We must suppose that when an atom of oxygen goes into combination, the energy released is of such form that it will excite adjacent atoms of oxygen into similar combinations. In the case of a gas engine we know that the heat produced by one atom of oxygen that combines, will cause adjacent atoms to enter into combination. The rise of temperature is very great. Heat starts combustion and combustion produces heat. In the organism we will say, eruption causes chemical reaction and chemical reaction produces eruption. When the first dynamic movement is made in the body of the chick in the shell we may believe that the reaction is of the primitive nature above described. We have now demonstrated the beginning of animal movement. This important step being made, we see that the next need of the organism will be means to control movement so as to adapt it to changes of environment.

By giving further thought to the matter, we perceive that it would be a great advantage to the organism if a certain kind of membrane should be evolved or developed that would act as a blockade against excitation. At the same time, it would permit the passage of combustible matter or oxygen or a mixture of the two. We might now have a surcharged mass on both sides of the membrane. We see then that an excitation of the mixture on one side of the membrane would start an eruption which would be arrested when it reached the membrane. After the reaction, which we will say is on the inner side of the membrane, we will suppose that the products of combustion will filter out through the membrane and a new supply of explosive mixture will filter in through the membrane so that in time the matter on the inner side will be ripe for a second eruption or reaction. Let us for convenience term such a membrane as this a "check wall."

Let us next suppose a surcharged mass that is surrounded and enclosed in a check wall and that on the outside of the check wall is another surcharged mass. Now if an eruption is excited within the inner mass, it will mobilize the entire inner mass but the mass outside of the wall will not be mobilized. The chemical reaction will be confined within the check wall. We will suppose that the eruption produces movement and expansion and that the check wall itself is made to move and that this causes some movement in the outer surcharged mass. After the eruption within the check wall there will be a period of shrinkage during which the excess products of combustion pass out through the check wall. There will then be a final period during which oxygen and combustible compounds pass in through the check wall and the remaining products pass out. A fresh surcharged mass will thus be formed ready for a second eruption and so completing a cycle. Let us call such a mechanism a "motor unit."

In order to be effective a motor unit must be so placed in the organism that it is somewhat protected from the environment so that as a rule there will be sufficient time for recovery between eruptions. Let us bear in mind that every motor unit is in such a protected position that energy is supplied infrequently only.

It is fair to assume that each motor unit is strengthened by exercise, for the movement in the outer surcharged mass as the result of an eruption within the unit, may be assumed to aid in the removal of products of combustion and the bringing up of fresh stores of oxygen and combustible matter.

We now have a mechanism for the release of energy. To make it more effective, we will suppose that the motor unit is provided with an outer enclosing membrane so disposed that an increase in volume within, causes a shortening in length such as we observe in contractile fibers. Remember now that a motor unit is supplied with fuel and oxygen from its own environment and it is so insulated that time is allowed for its mass to become surcharged.

Let us note again regarding the eruption in the motor unit that we do not know its exact nature. It is not a high temperature operation such as the explosion in a gas engine. It is a rearrangement of molecules with release of energy, partly as dynamic movement and partly as heat. It requires oxygen and carbon bearing matter. To do work, an animal must have air and food and carbon dioxide is always produced when work is done. In the higher animals, the arteries convey oxygen and combustible matter to the contractile tissues. The veins carry away the carbon dioxide formed in these tissues. In other words, we find that oxidation takes place in the tissues where energy is released. Our theory of the motor unit is therefore, well founded.

Let us now think of a number of motor units connected together in such fashion that the eruption of one unit will be communicated to the adjacent units until all are discharged while at the same time the group as a whole is protected from environmental disturbance for the greater part of the time. Let us call such a group of motor units a "motor group."

Let us next suppose that a stalk like structure is evolved in the intermediate zone. It would consist of a check wall in tubular form with a core of surcharged mass within the wall and a ring of surcharged mass around the tube. One end of the tube would lie at a point exposed to disturbance from the environment. At every disturbance an eruption would start at this end of the tube and be communicated to the other end. The tube would then contain the eruption products. In a short time, however, the eruption products would filter out through the check wall and a fresh supply of surcharged mixture would filter in so that the tube would be all set for a second eruption. For convenience, let us call the arrangement a "signal line." When there are such an immense number of forms produced in a plant life there is nothing improbable in the formation of a signal line as above described. It is also true that if the signal line should prove advantageous to the organism, it would become hereditary by natural selection. We may suppose further, that by aid of natural selection the signal line would become longer and longer. It might be that the tubular check wall would become narrower and narrower with succeeding generations as with the narrower tube, the recovery after an eruption would be more rapid and eruptions could occur with greater frequency.

We have described two primitive structures, the motor unit and the signal line. In each case we have a sort of reaction channel. We may suppose that a segregation takes place so that the channel makes its own parts just as the Mississippi makes its own alluvial banks. The reaction channel is like a river in another way, viz.: The stream is fed by numerous springs in its bed and banks and the surcharged mass of the reaction channel is fed by an inflow of fresh supply through the check wall.

No doubt, you perceived some time back that the contractile fibers found in animals are the same things as the motor groups in our demonstration and the nerve fibers in animals are the same things as the signal lines in the demonstration.

To proceed let us suppose a motor group that is connected to one or more signal lines. An eruption in the line will serve to excite the group. We see the combination is suitable for a behavior operation in which a stimulus from the environment provokes a movement of the organism. We may say this marks the first important advance above the plane of vegetation.

It is obvious that such a combination would usually be a distinct advantage to the organism in the struggle for existence. We may reasonably suppose that when the motor group reacts, it produces certain changes in the organism that are favorable to subsequent reactions. In other words the motor group will be strengthened by exercise. After a movement, an increase in combustible matter may be had and also an increase in the oxygen supply.

We may suppose that the process of natural selection would tend to preserve the motor group signal line system. It would also tend to improve the system step by step by variation and survival of the fittest. For instance, we can see how primitive sense organs might be formed. The parts of the organism adjoining the signal line might be modified so as to protect it from all but certain forms of energy. For example the primitive ear would be protected from chemical stimuli and from light and heat and impact but would still be open to stimuli from sound waves in the environment. The primitive eye would be protected from sound waves and heat and chemical stimuli but would be open to stimuli from light. The primitive olfactory organ would be protected from sound and light and heat but would be open to certain chemical stimuli such as are carried by the air. The evolution of bony structures would naturally aid in the evolution of sense organs as they would protect the sensitive mechanisms from impact from the environment.

In this connection let us remember that the motor group signal line system in its simplest form would enable a larger organism to compete successfully with a smaller one. In very small organisms the internal activity is directly affected by the environment.[3] A very small animal may have a simple structure and yet respond quickly to change in environment. Large plants respond slowly to change, having no mechanism for rapid communication.

The next important step in evolution that we will consider is when two motor groups occur in opposition so that the shortening of one group will lengthen the other. Let us for convenience call this arrangement a "paired motor group." It is something like a double acting steam engine. The advantages are obvious. The passive lengthening of the one group will be favorable to the quick removal of the excess products of its last eruption and will thus prepare it for a new eruption. The system could by easy stages become more and more effective.

Let us now suppose the system so perfected that an environmental disturbance of unit strength will cause the ignition of one-tenth of the motor units in a certain motor group and that a disturbance twice as strong will cause the ignition of two-tenths and so on in proportion so that the number of units ignited is in proportion to the strength of the igniting disturbance up to a certain limit. There would have to be a multiple signal line for the system. We see that the amplitude of the movement of the motor group as a whole, would be graduated to suit the strength of the exciting disturbance in the environment. Such a motor mechanism might well be of great advantage to an organism.

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[3] E. A. Kirkpatrick, Genetic Psychology. Macmillan Co., New York, 1909, p. 87.

There are many ways in which motor groups might be differentiated with advantage to the animal in the course of evolution. The evolution of cartilaginous and bony structures in connection with paired motor groups would aid in the development of such groups.

Let us not overlook the fact that as the species develops, the environment of the individual also develops. At the stage when organisms first had sense organs and signal lines, their social environment became a factor in their continued development. A change in the environment of one individual might be communicated to another individual and result in movement of the latter. Such operations were probably of some value for the adaptation of the species concerned.

The foregoing explanation of the development of nerve muscle mechanisms would be more convincing if we had working models. Fortunately one very good model is found at hand in the modern kitchen. In the gas cooking stove there are perforated pipes. When the gas valve is opened a mixture of gas and air escapes from the perforations. You touch a match to the nearest jet and a flash will run along the pipe. Close the valve and open it repeatedly. There will be no flash unless you hold the match to one of the jets. Whenever one jet is lighted, the flame runs the length of the pipe. The operation we see is much like that in a signal line as specified in our demonstration. The principal difference is that in the case of the stove, the surcharged mass as it were, is ignited on the outside of the tube while in the case of the signal line, the eruption is confined within the tube or check wall.

Our explanation has now reached a stage where nerve muscle mechanisms are fairly established. One way to test our theories is to try to go farther with them. Let us see if we can explain a further development of mechanisms such as are employed in behavior that can be called intelligent. We may say we have reached the dawn of intelligence. Let us go farther in the direction of greater intelligence. Supposing our organism to be provided with such mechanisms as signal lines and motor groups, what do you think would be the next betterment to transpire?

Suppose the check wall of a signal line to be modified at a certain point so that it will sometimes permit the passage out of the impulse. Suppose the structure to be such that a single eruption on the inside will not pass through but will modify the check wall so as to weaken its resistance. A succession of eruptions will break through. The structure acts as a gate that is closed to a single impulse but is open to a succession of impulses. Now suppose an incoming signal line coming from a sense organ to be joined to an outgoing signal line that leads to a motor group. At the point of junction, we will suppose, there is a gate like structure as above described. We see that a single eruption in the incoming line would be arrested at the gate but a second or third eruption might pass through. We will make a further assumption that after the passage of an eruption impulse the way will gradually close up again so that it will soon return to the original state and be closed against a single impulse. It is not unreasonable to assume that such a modification of the check wall as this could occur.

Let us for convenience call this connecting structure between two signal lines a "line junction." Bear in mind that after a period of rest, a single eruption will not pass through a line junction but several eruptions within a short time will open the junction and cause an eruption in the second signal line.

Now let us suppose another kind of a junction between two signal lines that operates quite differently from the "line junction." We will term such a junction a "guard junction." It is so constituted that a single impulse will pass through but any impulse following soon after it will be arrested. After a certain lapse of time, a new impulse can pass through. We see that in operation the guard junction is the reverse of the line junction.

We will now consider a signal line as modified in another fashion. Suppose we have a forked signal line so that there is a main or trunk line and two branch or tributary lines. We will further suppose that each tributary line begins at a guard junction. We see that the same tributary can not bring two impulses in close succession but an impulse may come from the second tributary right after an impulse from the first tributary. There will be two impulses in succession in the trunk line if the two branch lines are discharged in sequence. The two eruptions in succession can only occur by the co-operation of the two branch lines. Let us call such a system as this an "association line."

Let us now suppose a new combination. We will have an association line where the trunk line terminates in a line junction that joins it to a "motor line" that leads to a motor group. We see that if an eruption occurs in one of the branch lines, the impulse will be conducted through the trunk line to the line junction where it will be arrested. If, however, it is followed by an eruption in the second branch line, this latter impulse may successfully pass through the line junction and produce an eruption in the motor line that will be conducted to the motor group where a movement will be provoked. After this occurrence, the line junction will remain open for a time and we perceive that if the guard junctions permit, an impulse from one branch may pass through and make a movement at the motor group.

Looking at such a system from a different angle, we find that line junctions and guard junctions may be regarded as points of high resistance to the passage along signal paths, of eruption impulses.

If you think the matter over, you will perceive that an association line as above described will be an effective instrument for intelligent behavior. When the first association line transpired, there was the beginning of learning. The association line is an essential mechanism for associative memory. Let us think of a special case. Suppose one branch of an association line connects with an olfactory organ and one branch, with an organ of taste. After proper experience, an impulse excited in the olfactory organ may cause the same movement as an impulse coming from the taste organ.

It should be remembered that in the animal organism there would be a number of similar association lines that would act together. Note also that some signal lines will have diverging branches leading to different motor groups. Each branch will have its junction with a connecting signal line. Thus we get diverging paths for eruption impulses. Such paths are of obvious utility for selective responses. An exciting change at one sense organ may produce an eruption in two or in many association lines.[4]

Having thus outlined the evolution of mechanisms for associative memory, let us go still further and seek an explanation of mechanisms which serve to register changes in the environment of the subject and of how these mechanisms are evolved. In the primitive organism before nervous mechanisms existed, there were no doubt molecular changes in response to changes in the environment. A change of temperature for example, excited a certain molecular response. A change in light conditions excited a different molecular response. There was a characteristic response for sound waves and another for chemical changes in the medium in contact with the subject. In other words the organism was sensitive to heat, light, sound and chemical changes. In plants we find today a similar sensitivity except in the case of sound. As the organism developed in structure, sense organs were evolved as we have already indicated. Signal lines also were evolved leading from the sense organs.

Now we will suppose that "signal centers" were evolved along with the signal lines. When an igniting impulse began in a sense organ it was propagated along the signal line to the signal center where it provoked a certain molecular response. We will assume that this response was of variable quality and that the particular quality was determined by the quality of the disturbance in the environment. By way of explanation we will state that in the primitive signal line, the eruption would have a quality depending on the form of energy in the environmental disturbance which excited it. For instance, a change of temperature would cause an eruption of a certain quality. A change in light conditions would cause an eruption of a different quality. In other words, the molecular movement or response would be different for light and heat. We will say that as the signal lines developed they gradually became more and more alike but that the signal centers retained there correspondence with the origin of the eruptions reaching them. Where a heat receptor was developed at the outer end of a signal line, a heat center was evolved at the inner end. Where a light receptor was developed at the outer end, a light center was evolved at the inner end. In the same way were evolved centers for sound and chemical changes and for impact or pressure changes. As a result, the response in every signal center would have a certain correspondence to the form of energy that provoked it. Every signal center we see, became a registering mechanism.

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[4] The functions of association nerve paths and diverging nerve paths are discussed by the writer in the following articles in the Psychological Review, The effect of high resistance in common nerve paths, 1916, 23, 231–236. Compound substitution in Behavior, 1917, 24, 62–73.

Let us proceed by supposing two signal lines connected in series, each line having a signal center and the signal center of the first line which we will call A being connected by a line junction with the outer end of the second line which we will call B. We will assume that the molecular response in center B is always of the same quality as that in center A. If A is a light center B is a light center. In the same way we could have a number of signal lines in series so as to form a long path for eruption impulses. The signal centers would be stations on the path and each center would respond in turn and pass the signal along to the next one. Let us now suppose that at the inner end of the path there is a "head center." We will also suppose that all the head centers for light are grouped together and all the head centers for sound are grouped together and so on. Every sense organ will be in communication with its head centers and a change at any sense organ will be simulated in its head centers. All changes in the environment will be registered in the head centers.

To present the matter differently, we will say that certain signal centers are tuned to sound and so when eruption is excited in them the molecular response is the same as if sound waves had penetrated the centers, while certain other centers are tuned to light and so when they are excited, the molecular response is the same as if a ray of light had penetrated these centers. We observe that the process in a signal center for sound in effect reverses the process that occurs in the auditory sense organ. The process in a signal center for light reverses the process that occurs in the visual sense organ where the eruption has its beginning.[5]

To illustrate further, it would not be unreasonable to suppose that in a heat center there is an actual rise of temperature like a little fever at each eruption; that in a center for smell or taste, a secretion or a chemical change is provoked by an eruption; that in a tactile center, there is an actual dynamic movement such as a tiny swelling, at each eruption.

The result is something like the strings of a musical instrument where each string gives when struck, a particular note. The development of registering mechanisms in the manner outlined might be of some aid in adapting the organism to its environment and so it would be preserved by natural selection.

Limitations of space will not permit a full discussion regarding molecular responses and molecular structure as required for our assumptions covering registering mechanisms. In a general way we make the assumption that animal matter can vary sufficiently in molecular structure to give enough variation in quality of molecular response to provide for such correspondence between signal centers and environment as represented at the sense organs, as will suit our demonstration of registering mechanisms.

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[5] The writer has previously outlined the theory used here in his article Communication, correspondence and consciousness, Psychological Review, 25, 344.

In the preceding discussion we have traced the development of nerve muscle mechanisms. We have used special names for the different structures that were developed. It may make the whole matter clearer to you if we compare these special terms with equivalent terms in general use regarding the nervous system in man and animals. The structure termed a signal line will be recognized as the equivalent of a nerve fiber, etc.

Equivalent terms

Signal line	Nerve fiber
Motor group	Muscle fiber
Line junction	Synapse
Guard junction	Synapse
Association line	Association nerve fiber
Signal center	Nerve center or spinal center
Head center	Brain center or cortical center

To those not familiar with the terms in the second column it should be said that the synapses are the junctions between nerve fibers and are thought by some authorities[6] to have variable resistance to the passage of impulses.

The mechanisms of the higher mental processes and the higher brain centers are located by authorities among the nerve cells in the cerebral gray matter known as the cortex. When a man is thinking, the brain cells in the cortex are in activity.

Some readers may be given a better grasp of the subject if a diagram is given to represent the different structures referred to in the preceding discussion. Figure 1 is such a diagram. The arrows indicate the direction of the eruption wave.

It is quite obvious that in tracing the development of nerve muscle mechanisms, we must keep in mind the evolution of other organs and the evolution of the individual as a whole. As an illustration, we know that at one stage of evolution, the highest form of animal was like a caterpillar, made up of a chain of similar segments. Each segment had a certain individuality of its own. The nerve muscle mechanisms of each segment were under separate control to some extent. Each segment had its own segmental head centers. Signal lines

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[6] W. B. Pillsbury, Essentials of Psychology. New York, 1912, pp. 50–55.

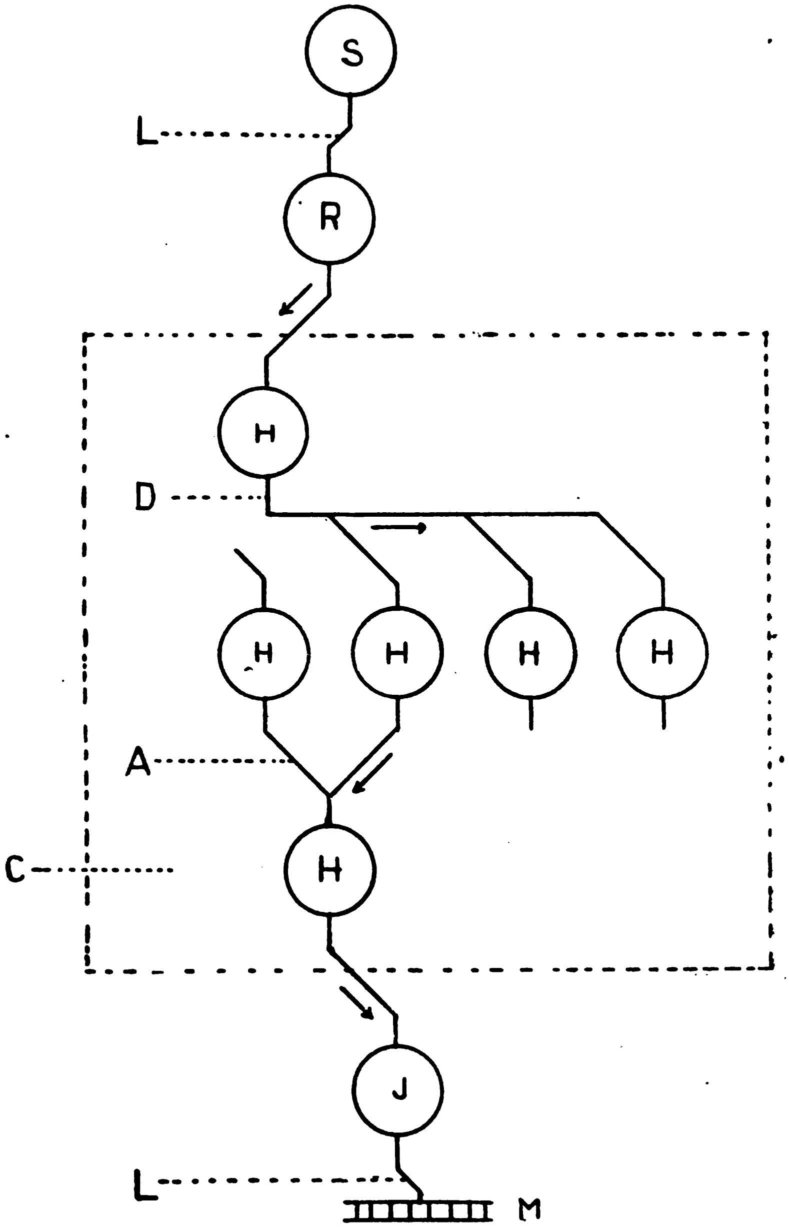

FIG. 1. DIAGRAM SHOWING DIFFERENT STRUCTURES

S, sense organ; *L*, signal line; *R*, signal center; *H*, head center; *D*, diverging line; *A*, association line; *J*, line junction; *M*, motor group; *C*, head center area.

connected the segmental head centers. This accounts, we observe, for the nervous architecture of vertebrate animals with its spinal system of nerve centers.

Taking a brief review of our demonstrations, we see that the primitive organism reacted to changes in its environment. The responses were in the form of molecular changes not confined to definite areas. It came about that the intermittent release of energy in molecular changes in certain areas, produced movement. Periodic energy releasing cycles causing movements were established. It only remained to develop structures which would confine energy releasing changes to more prescribed areas. In this way arose motor organs, paths of communication and sense organs. Further differentiations and development produced structures that served to condition responses so that behavior was determined by individual experience. Along with the differentiation of sense organs there came also a differentiation of areas having special sensitivity, giving mechanisms that register in correspondence with the environment. It is by means of such mechanisms that a man is conscious of his environment.

NOTES ON LIGHT DISCRIMINATION IN THE DOG[1]

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I. INTRODUCTION

1. Statement of problem. This investigation was undertaken for the purpose of determining the threshold of brightness discrimination in the dog. The threshold is arbitrarily defined as the least difference in illumination of a standard and a variable light which the dog can discriminate without error in thirty consecutive trials.

2. History of the present study. The study was conducted in the Psychological Laboratory of Indiana University during the year 1915. Four dogs were used at the outset of the experiment: a thoroughbred cocker spaniel, age three and one-half years; two thoroughbred fox terriers, age four months at the beginning of the experiment; and a mongrel, about six years of age. Soon after the experimental work was initiated the cocker spaniel contracted a severe case of distemper which made him useless for further study. The mongrel failed to form the discrimination habit and, finally, after a long period of fruitless training, was discarded. The results to be presented herein were taken, for the most part, from the study of the fox terriers, which proved to be fairly satisfactory subjects.

Following the work with the dogs, ten human subjects were tested in the discrimination problem under conditions of discrimination similar to those set up for the dogs. The results of only three of them are relevant to this study. From these tests comparative data on the limits of discrimination were obtained.

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[1] This study was suggested and directed by Dr. M. E. Haggerty, to whom I am indebted for the use of technique and apparatus devised and employed by him in an unpublished study of light discrimination in the dog.

3. Previous studies in discrimination in the dog. Vision in the dog has been studied more extensively than any other sensory function. Experiments have been conducted to test its ability to discriminate colors, brightness differences, size, form, and patterns.

In many of the early studies on color vision in dogs the experimenters failed to differentiate brightness from hue; hence conclusions drawn as to the presence or absence of color vision are unreliable. The studies of Graber,[2] Gates,[3] Himstedt,[4] and Nagel are subject to this criticism.

Colvin and Burford,[5] attempted to control the brightness factor in testing for color vision, but their control rests on the assumption that the colored papers used have the same brightness values for the dog as for man. The validity of this assumption has been questioned by other experimenters because of the lack of experimental data to substantiate it. Their results which indicate that dogs unquestionably discriminate colors and that they have remarkable keenness in fine color discrimination are, therefore, inconclusive in the absence of adequate control of the brightness factor.

Probably the best study that has been reported on color discrimination in the dog is that of Smith.[6] In this investigation the brightness factor was controlled by using colored papers of equal brightness as tested by the flicker method. Seven dogs were used in the experiment, but only one completed the whole series of tests. Zimmermann's colored papers and Nendel's grays were used as stimuli. From the results of the study Smith concludes that the dogs seemed to have a very imperfectly developed sensitivity to color differences: and that, owing to the rudimentary character of this sensitivity and the instability of reactions to colors, this sensitivity can play only an unimportant or secondary rôle in the life of the dog when other factors such as brightness or position are operating.

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[2] Graber, Vitus. Grundlinien zur Erforschung des Helligkeits und Farbensinnes des Tiere.

[3] Gates, Elmer. The Science of Mentation.

[4] Himstedt, V., and Nagel, W. A. Versuche über die Reizwirkung verschiedener Strahlenarten auf Menschen und Tieraugen.

[5] Colvin, S. S., and Burford, C. C. The Color Perception of Three Dogs, a Cat, and a Squirrel.

[6] Smith, E. M. Some Observations Concerning Color Vision in Dogs, p. 119.

Orbeli,[7] using the method of Pawlow, found no evidence to support the belief that dogs can discriminate on the basis of color.

Up to the present time studies on the keenness of brightness discrimination have given us only a vague idea of the degree of sensitivity of the dog's eye to differences in brightness. Discrimination of gray papers has proven to be too crude a method for establishing threshold values, although it serves very well to demonstrate the fact that the dog can discriminate readily on the basis of brightness differences. Field observations also substantiate the view that the dog is sensitive to slight changes in brightness and indicate that this sensitivity plays an important rôle in the daily life of the dog.

Pattern discrimination has recently been tested by Johnson[8] by means of a modified form of the Yerkes-Watson brightness apparatus. The results of his experimental work were inconclusive; the experimenter thinks, however, that the dogs indicate a low degree of sensitivity to details in pattern.

The chief facts that have been gathered by the various kinds of experimental work in discrimination in the dog may be briefly summarised as follows: Contrary to popular opinion, the dog possesses only very rudimentary sensitivity to colors and depends very little, or not at all, on color distinctions in daily life. The dog readily detects slight differences in brightness and is able to discriminate between objects differing only in form and in size. The dog is probably not highly sensitive under laboratory conditions of experimentation to details of pattern such as those presented by the Johnson-type of experiment.

II. TECHNIQUE

1. Description of apparatus. The apparatus used to test the discrimination of dogs consisted of two parts: the Yerkes-Watson brightness apparatus;[9] and a control box[10] essentially similar, in general plan, to that described by the authors, but built especially for the testing of dogs and adapted to the needs of the present experiment. The plan of the control box is shown in figure 1.

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[7] Orbeli, L. A. Réflexes conditionnels du côté de l'oeil chez le chien.

[8] Johnson, H. M. Visual Pattern-Discrimination in the Vertebrates.

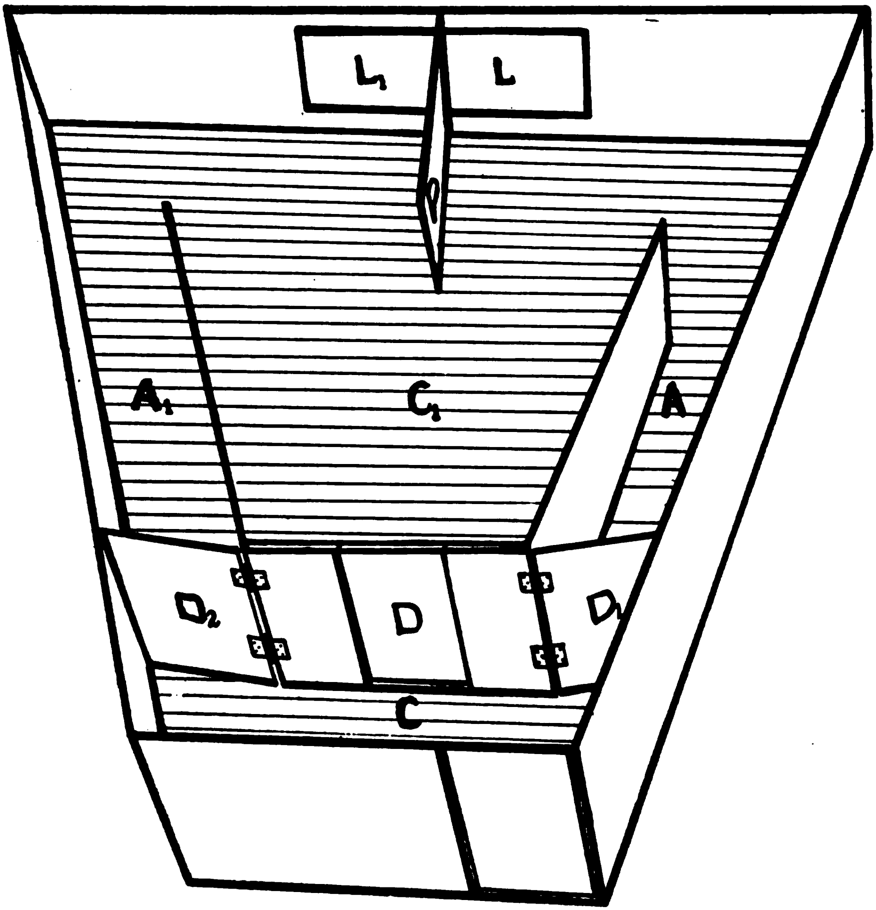

FIG. 1. CONTROL BOX

The frame of the box is rhomboidal in shape with its non-parallel sides equal in length. Its sides, partitions, and framework or cover are made of white pine lumber; the floor, of seasoned walnut. The top and front wall are covered with one half inch mesh wire netting. A heavy black broadcloth screen is interposed between the experimenter and the front of the box to prevent the dog from seeing the former during the time of discrimination. All parts of the box are painted with oil and lampblack of a dull velvet black shade. Light reflected from the walls of the cage is barely perceptible to the human eye. With less than 1 c.p. difference in the illumination of the two light windows, however, the difference in the amount of reflected light in various parts of the cage is wholly imperceptible to the human eye.

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[9] Yerkes and Watson. Methods of Studying Vision in Animals.

[10] Devised and used by Dr. M. E. Haggerty. Study unpublished.

On the floor of the box is a punishment grill consisting of oxidized copper wires stretched parallel with each other. These wires are attached at one end to the wooden framework of the floor of the box and at the other end to a steel plate extending along it. At each end they are alternately attached to the wood and to the metal plates. The plates are in circuit with the terminals of an induction coil whose primary is connected with a storage battery. A switch placed in the circuit at a point convenient for operation by the experimenter enables him to give a slight electric shock to an animal standing on the grill at any place in the control box. The floor of the box is covered with velvet black broadcloth, the surface of which reflects very little light.

The control box is divided into four compartments: a starting compartment (*C*); a discrimination chamber (C_1); and two alley-ways (*A* and A_1). By way of the latter, the dog may return to the starting compartment after leaving the discrimination chamber. The entrance to the starting compartment from the alley-ways is controlled by Doors D_1 and D_2. Entrance to the discrimination chamber is effected through the doorway at *D*. Here there is a sliding door made of a heavy metal plate enclosed in a wooden frame. When the metal plate, which, in place, completely obstructs the passage way, is released it sinks noiselessly in its frame to a position beneath the floor of the box. Partition prevents rays of light from the two light windows L and L_1 from mingling until at a distance of approximately 20 inches from the windows. This partition fits snugly against the stimulus adapter of the light box and stands midway between the windows.

2. Calibration of lights. The light sources in the light box were 225-Watt, 110-volt Mazda lamps. Throughout the investigation a constant voltage of 105 was maintained. The intensity of illumination of one of the two light windows was held at 1 c.p. (Hefner units). In the discussion that follows this window is referred to as the *standard light.* The other window was always illuminated with an intensity greater than 1 c.p. and, in the course of the experiment, was changed by small amounts so that its intensity, with each adjustment, more nearly approximated that of the *standard light.* To this window the name *variable light* is given. During each discrimination trial the light window on one side of the light box was illuminated with an intensity of 1 c.p.; on the other side the window was illuminated with an intensity somewhat greater.

By the use of a Leonard-Weber photometer[11] the position was determined for each lamp in the light box at which it illuminated the light window with an intensity of 1 c.p. in a perfectly dark room. With this position known for each lamp, it was then possible to calculate, by the usual photometrical formula, the position that would illuminate the window with any other desired intensity. From time to time the intensities of the light windows was determined to guard against slight changes in the intensities of the light sources arising from deterioration through use.

3. Training the dogs to discriminate. A great deal of care and time was devoted to the early training periods in order to establish stable habits of discrimination. *The success of such experiments with animals depends in large measure upon the persistence of discrimination habits when the brightness differences are small and therefore not easily discerned.*

The first step in the training process was that of teaching the dog to make, immediately upon the release of the door at D

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[11] Stine, Wilbur M. Photometrical Measurements, p. 80.

(see fig. 1) a circuit—leaving the starting compartment, going through the discrimination chamber, into one of the alleys, and thence returning to the starting compartment. While this habit was being formed the illumination of the light windows was kept identical. All lights in the room were turned off and such noises as those made by the shifting of apparatus and the induction coil were introduced at this time in order that the dog might become thoroughly accustomed to them.

When this habit was thoroughly established, the lights in the light box were so adjusted that the illumination of the light windows presented a marked difference in intensity—approximately 35 c.p. The dog was then trained to go to the alley on the side of the darker light. The doors at D_1 and D_2 were opened to allow his return from the alleys to the starting compartment only on the side of the *standard light.* When he chose the wrong alley he was punished by means of the grill and made to turn back and enter the alley on the side of the *standard light.*

The standard and the variable lights were shifted from side to side according to a rather complicated schedule made out in advance of the training and followed throughout the study. The order of shifting was of such complexity that the animal could not learn this order and thus avoid the necessity of discrimination in making the correct responses. Table 1 gives the order of tests used. The trials were given in series composed of fifteen tests. A complete set of trials consisted of twelve such series so arranged that the standard and the variable lights appeared an equal number of times on either side. When a complete set of trials had been given, the set was repeated, beginning with the first series and running through the entire set as before. Each day, excepting Sunday, from two to three series (30 to 45 trials) of tests were given each dog. Testing was usually done between the hours of 2 and 5 p.m.

Care was exercised to prevent the animal from detecting the position of the standard and the variable light on any basis other than the difference in illumination. The experimenter was not visible to the dog while discriminations were being made. Noises involved in the shifting of the lights was reduced to a minimum, and, prior to each trial, the same amount of shifting was done whether or not the actual positions of the lights were changed. To insure thorough darkness adaptation the dogs were confined in the dark room for a period of from ten to fifteen minutes before the first discrimination test was given.

After the animal had perfected the habit of choosing the alley-way on the side of the standard light so well that thirty consecutive errorless discriminations were made, this habit was employed in finding the threshold of discrimination. The lamps in the light box were so placed that the intensity of the *variable* light was 3 c.p. giving an intensity difference of 2 c.p. between it and the *standard light.* The dogs were required to choose the alley-way on the side of the *standard light* as in the preliminary training period and were punished (electric shock) for failures. They were required to make consecutively thirty errorless discriminations before proceeding from one problem to another problem in which the difference in the illumination of the two lights was somewhat reduced. They worked independently and were given problems involving a lower discriminable difference as soon as a given problem had been completed.

TABLE 1

Position of the standard light during 180 trials

L = Standard light on left side. R = Standard light on right side

R	L	R	R	L	L	R	L	R	L	L	R	R	L	R
L	R	L	L	R	R	L	R	L	R	R	L	L	R	L
R	R	L	L	R	L	R	R	L	L	R	L	R	L	L
L	L	R	R	L	R	L	L	R	R	L	R	L	R	R
R	L	R	R	R	L	L	L	R	L	R	R	L	L	R
L	R	L	L	L	R	R	R	L	R	L	L	R	R	L
R	R	L	L	R	R	L	L	R	L	R	R	L	L	R
L	L	R	R	L	L	R	R	L	R	L	L	R	R	L
R	L	R	R	R	L	L	R	R	L	L	L	R	L	R
L	R	L	L	L	R	R	L	L	R	R	R	L	R	L
R	L	R	L	R	R	L	L	R	R	L	L	L	R	R
L	R	L	R	L	L	R	L	L	L	R	R	R	L	L

III. RESULTS

1. *Fixation of discrimination habit.* Table 2 gives a summary of the trials and the responses given by the mongrel and the

two fox terriers during the training period required to establish the habit of discrimination.

The young female showed evidence of discrimination after approximately 225 trials, but 150 additional trials were required to so perfect the discrimination habit that thirty consecutive errorless choices could be made. The young male required more trials than the female before signs of discrimination appeared. The first clear evidence of his discrimination came after 345 trials. There were given 210 additional trials before thirty errorless choices were made. The mongrel failed completely to give signs of discrimination during 720 trials. A few days after training was begun, he showed a tendency to go to the left alley irrespective of the position of the standard light. This tendency became habitual and persisted until the course of training for this animal was ended. The persistence of this *left-alley* habit resulted in a relatively larger number of errors on the left side than on the right side, as is shown in the table of errors of this animal. Of a total of 351 errors, 82 per cent were made by his going to the left alley and only 18 per cent by going to the right side. An alternation habit or random choice without discrimination would have resulted in approximately 50 per cent of the errors being made on each side.

TABLE 2

Summary of trials given leading to the fixation of the discrimination habit

DISCRIMINATION DIFFERENCE	ANIMAL	TOTAL NUMBER OF TRIALS	RIGHT CHOICES	WRONG CHOICES	WRONG ON RIGHT	WRONG ON LEFT	PER CENT OF ERROR
35 c.p.	Young female..	375	264	111	55	56	29.8
	Young male...	555	361	194	143	51	35.0
	Mongrel.......	720	369	351	63	288	48.7

It was necessary to give a large number of trials to the young animals after the first evidence of discrimination was manifested before thirty consecutive errorless choices were made. The records for the last 150 trials for each of the three dogs is given in table 3. An average of approximately one error for each series of 15 trials was made during the last ten test series in the case of the young animals. The number of errors during the

TABLE 3

Records of the last 150 trials

ANIMAL	NUMBER OF TRIALS	RIGHT CHOICES	WRONG CHOICES	WRONG ON RIGHT	WRONG ON LEFT	PER CENT OF ERROR
Young female	150	139	11	2	9	7.3
Young male	150	140	10	5	5	6.7
Mongrel	150	182	68	9	59	45.4

TABLE 4

Summary of discrimination records for female dog. Thirty consecutive errorless choices taken as criterion of discrimination with each intensity. Chose standard (darker) light. Intensity of standard light is 1 c.p. throughout the descent

DIFFERENCE IN INTENSITY OF STANDARD AND VARIABLE LIGHTS (HEFNER UNITS)	NUMBER OF TRIALS	RIGHT CHOICES	WRONG CHOICES	WRONG ON RIGHT	WRONG ON LEFT	PER CENT OF ERROR
2.0 c.p.	225	200	25	14	11	11.1
1.5	105	100	5	2	3	4.7
1.0	90	86	4	1	3	4.4
0.5	45	43	2	1	1	4.4
0.4	330	272	58	22	36	17.2
0.3	315	208	107	33	74	33.9

Position habit established with total loss of discrimination habit at a difference of 0.3 c.p. Returned to a difference of 0.5 c.p. to re-establish the habit

0.5	45	40	5	4	1	12.5
0.4	285	239	36	17	29	16.1
0.375	75	67	8	4	4	10.6
0.350	75	66	9	2	7	12.0
0.325	30	30	0	0	0	0.0
0.3	60	54	6	4	2	10.0
0.28	90	77	13	4	9	14.4
0.26	60	54	6	2	4	10.0
0.24	75	66	9	7	2	15.0
0.22	60	56	4	2	2	6.6
0.2	90	82	8	7	1	8.9
0.18	60	553	7	4	3	11.6
0.16	90	78	12	7	5	13.3
0.14	105	87	18	12	6	17.1
0.12	300	201	99	52	47	33.0

Descent stopped at this difference. Apparently near the point of just perceivable difference. Returned to 14 c.p.

0.14	210	173	37	27	10	17.6

last 150 trials with the mongrel are about the same as the number at any time preceding. There is no evidence of fixation of the discrimination habit in his case.

2. Discrimination tests with female dog. Table 4 gives in summary form the records of the female dog for each discrimination series (15 trials) with problems graded in difficulty from a discriminable difference of 2 c.p. to a difference of 0.12 c.p.

The habit of discrimination broke down when this animal was discriminating a difference of 0.3 c.p. After 315 trials had been given without indication of her regaining the discrimination habit in sufficiently stable form to insure her making thirty perfect choices, it seemed advisable to return to a simpler problem. The difference in the intensities of the standard and the variable lights was raised to 0.5 c.p., a difference that she had previously discriminated with ease. During the first series of 15 tests, after the change in intensity, she regained the discrimination habit, and discriminated the next two series without error. Hereafter the amount of reduction of the intensity of the variable light, at each succeeding step toward the threshold, was made very small. Under this condition the habit of discrimination persisted, with only temporary disruptions, until a difference of 0.12 c.p. was reached. The habit, at this problem, became less stable than usual and appeared to be on the verge of breaking down. To avoid the latter, the difference in intensity was raised to 0.14 c.p.—the least difference previously discriminated. She again made thirty perfect discriminations at this difference. It is evident from these results that the least discriminable difference between the standard light with an intensity of 1 c.p. and the variable light of greater intensity is less than 0.14 c.p. for this dog. It is probably as low or lower than 0.12 c.p. For if the discrimination habit had remained stable with this latter difference it is almost certain that thirty perfect choices would have been made after further practice.

Evidence of her ability to discriminate a difference of 0.12 c.p. is found in the detailed records of her early discriminations, when the habit was relatively stable, at this difference in intensity. In one series of 15 trials the per cent of error was but

15.3. In three such series 20 or less per cent of error was made. In two consecutive series 23 of the 30 choices were correct, giving a per cent of error of 23.3 In three consecutive series (45 trials) the per cent of error was 25.6. Random choice would not give that degree of accuracy. In 75 consecutive trials the per cent of error was 26.5—a per cent which closely approximates the criterion for positive discrimination set by Yerkes in his study of the dancing mouse. His criterion is defined in the following quotation: "Anything (referring to errors) below 40 per cent is likely, however, to be the result of ability to distinguish the brighter from the darker box. To be on the safe side we may agree to consider 25 wrong choices per 100 as indicative of a just perceivable difference in illumination. Fewer mistakes we shall consider indicative of a difference in illumination, which is readily perceivable, and more as indicative of a difference which the mouse cannot detect."[12]

If this criterion is applied to her records of 0.12 c.p. intensity one may safely conclude that the just perceptible difference is somewhat lower than 0.12 c.p.

3. Discrimination tests with male dog. Table 5 presents a summary of the male's records for a series of discrimination tests approaching the threshold of discrimination.

The habit of discrimination broke down three times during the work with this dog and finally became so unstable for problems involving difficult discriminations that the work was brought to an end before the least perceptible difference was found. The first disruption of the habit came while he was attempting to discriminate a difference of 0.1 c.p. Upon returning to a difference in intensity of 0.5 c.p., his confusion was still manifest; hence the difference was raised to 1 c.p. After five series of trials with gradually decreasing numbers of errors, two perfect series were discriminated and the descent toward the threshold was renewed. The habit of discrimination persisted this time until a difference of 0.4 c.p., was attempted; here again it broke down. Upon returning to the old problem of 1.c.p. difference, the habit was re-established only after a long period of training.

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[12] Yerkes, R. M. The Dancing Mouse, p. 123.

TABLE 5

Summary of discrimination records for male dog. Thirty consecutive errorless choices were taken as the criterion of discrimination with each difference in intensity. Chose standard (darker) light. Intensity of standard light was 1 c.p. throughout the descent

DIFFERENCE IN INTENSITY OF STANDARD AND VARIABLE LIGHTS (HEFNER UNITS	NUMBER OF TRIALS	RIGHT CHOICES	WRONG CHOICES	WRONG ON RIGHT	WRONG ON LEFT	PER CENT OF ERROR
2.0	330	253	77	37	40	23.3
1.5	75	71	4	3	1	5.3
1.0	105	94	11	7	4	10.4
0.5	75	70	5	1	4	6.6
0.4	90	83	7	2	5	7.7
0.3	30	30	0	0	0	0.0
0.2	105	100	5	1	4	4.7
0.1	225	139	86	60	26	38.2
Position habit formed; returned to difference of 1 c.p.						
1.0	235	110	25	21	4	18.5
0.5	75	65	10	4	6	13.3
0.4	270	196	74	51	23	27.4
Position habit formed; returned to difference of 1 c.p.						
1.0	330	295	35	16	19	11.8
0.5	60	55	5	2	3	9.0
0.45	285	239	46	17	29	16.1
0.4	105	87	18	9	9	17.1
0.375	30	30	0	0	0	0.0
0.350	105	87	18	12	6	17.1
0.423	60	54	6	3	3	10.0
0.300	45	44	1	0	1	2.0
0.280	135	110	25	13	12	18.6
0.260	150	136	14	9	5	9.6
0.240	105	93	12	9	3	11.4
0.220	450	299	151	116	35	33.3

Position habit formed at this difference. Problem discontinued because of growing instability of habit of discrimination

Henceforth the steps in the reduction of the difference in the brightness of the two lights was made very small in order to make new problems less difficult than they had been in previous descents toward the threshold. Excepting several short time lapses, the habit of going to the darker light was maintained in the third descent until a difference of 0.22 c.p., was reached. At this difference the habit became very unstable and eventually broke down completely. He was not able to make consecutively thirty perfect choices in 450 trials.

It would have been necessary to retrain him on an easier problem to have again established the habit of discrimination. But because of the increasing difficulty of maintaining the discrimination habit it was believed that the value of a fourth descent would be insufficient to justify further discrimination tests with this subject.

During the first descent toward the threshold, he seemed to have reached the approximate limit of discriminability at 0.1 c.p. The detailed records for the first descent show that this dog was able to discriminate a difference of 0.1 c.p., even though unable to make thirty consecutive errorless choices. In the first 20 consecutive choices he made only 20 per cent of error. In the first 60 choices he made 25 per cent of error. And in the first 90 tests the per cent of error was 27.7 This per cent of error closely approximates the criterion used by Yerkes[13] as an index of the just perceivable difference. The increasing number of errors from the beginning of this problem is due to the gradual weakening of the discrimination habit, which broke down entirely soon after the first 90 trials.

From these results it seems safe to conclude that the male dog could discriminate a difference in intensity of illumination as low as 0.1 c.p. At this difference, however, discrimination is difficult and can be done only under exceedingly favorable conditions in which the habit of discrimination is very stable. It is not possible to state the threshold of discrimination as defined at the outset of this experiment on account of the occasional errors of discrimination which made it impossible to secure 30 consecutive errorless choices before the habit broke down. From the data at hand, however, it may be assumed that the threshold is near 0.1 c.p. with the intensity of the standard light at 1 c.p.

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[13] The Dancing Mouse, p. 123.

IV. COMPARATIVE STUDY OF LIGHT DISCRIMINATION

1. Discrimination in man. Three human subjects were given extensive tests in brightness discrimination under conditions identical with those observed in the testing of the dogs and with the intensity of the standard light at 1 c.p. Each subject discriminated readily a difference of 0.11 c.p. for thirty consecutive trials. Two of the subjects failed to make 30 perfect choices with a difference of 0.1 c.p. intensity during 150 trials. The other subject succeeded in reaching this standard of accuracy with a difference of 0.09 c.p., but failed (150 trials) at a difference of 0.08 c.p. In subsequent tests with a difference of 0.06 c.p. each subject made less than 20 per cent of error in 45 consecutive trials. One subject made, in 45 trials, only 17.7 per cent of error with a difference of 0.04 c.p; with this difference, the per cent of error of the other subjects rose to approximately 35. From these results it is evident that the human subject can discriminate much smaller differences than the dogs could be trained to discriminate.

2. Comparison of least brightness differences discriminated. The so-called thresholds of visual discrimination that have been reported for animals are really only crude approximations of the true limits of just perceivable differences in illumination. As we have intimated heretofore, the usefulness of the discrimination method for the determination of the brightness threshold is dependent in large measure upon the stability and persistence of discrimination habits in the animals at times when differences in visual stimuli are so small that discrimination is difficult. Up to the present time the reported thresholds have not been adequately checked by other methods of experimentation. On this account we cannot definitely say whether the reported thresholds really represent the just perceptible difference in illumination or whether they merely denote the difference at which discrimination is sufficiently difficult to cause disruption of the discrimination habits.

At the present time we can make only rough comparisons of the least brightness differences discriminated by different animals because of the difficulty of equating the differences in experimental procedure. Probably the most comparable results have been gotten by the use of the Yerkes-Watson type of light apparatus. Results gotten by this mode of presenting the brightness stimuli will be considered in the following comparison. The ratios of the intensity of illumination of the standard and the variable lights has been taken as the best available basis of comparison. The validity of this basis of comparison rests on the assumption that Weber's law holds for animals.[14] In table 6 are given the conditions under which the thresholds to be compared were obtained. In this table are indicated the number of correct trials and per cent of accuracy, taken as criteria of discrimination. It gives also the intensity of the standard light and states whether the intensity of the variable was greater or less than that of the standard.

TABLE 6

Conditions under which the thresholds of light discrimination were determined for man, the dog, the English sparrow, and the dancing mouse

SUBJECT	NUMBER OF TRIALS INDICATIVE OF DISCRIMINATION	PER CENT OF ACCURACY REQUIRED	INTENSITY OF STANDARD LIGHT	INTENSITY OF VARIABLE LIGHT
			c.p.	
Human S, H, W*	30	100	1.0	Greater than 1 c.p.
Human A, B, C†	30	100	0.09755	Both greater and less than 0.09755 c.p.
Dogs*	30	100	1.0	Greater than 1 c.p.
English sparrows†	30	100	0.09755	Both greater and less than 0.09755 c.p.
Dancing mouse‡	100	75	5.0	4.5 c.p.

* This study.

† Tugman, Eupha Foley. Light Discrimination in the English Sparrow, pp. 85–88.

‡ Yerkes, R. M. The Dancing Mouse.

Table 7 presents the ratios of the intensities of the standard and the variable lights as discriminated by six human subjects, two dogs, four English sparrows, and one dancing mouse.

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[14] Watson, J. B. Animal Behavior, p. 359.

As shown by the ratios, the threshold for man is the lowest. Owing to the use of a somewhat more lenient criterion for discrimination in the mouse the ratio for this animal is somewhat larger than it would have been according to the criterion used in obtaining the ratios for the other animals considered. If the criterion of Yerkes is applied to the results of subjects S, H, and W, their best discrimination records would be represented by the ratios 9.61: 10 for S and 9.43: 10 for H and W. This same criterion applied to the results of the dogs gave a ratio of 8.9: 10 for the female and 9:10 for the male. Similar ratios for the other human subjects and the sparrows are not available, but it is almost certain that they would be somewhat larger than those given in the foregoing table.

TABLE 7

Comparison of thresholds of discrimination

In this comparison the ratio of the intensities of the darker and the brighter lights is taken as the basis of comparison

GROUP	SUBJECT	RATIOS OF INTENSITIES OF LIGHTS (INTENSITY OF DARKER LIGHT: INTENSITY OF BRIGHTER LIGHT)
Man	S	9.17 : 10
	H	9.01 : 10
	W	9.01 : 10
	A	8.6 : 10
	B	9.1 : 10
	C	8.6 : 10
Dog	A	8.7 : 10
	B	8.3 : 10
English sparrows	Male IV	8.4 : 10
	Female V	6.5 : 10
	Male V	6.9 : 10*
	Female VI	7.7 : 10
Dancing mouse		9.0 : 10

* Died at this ratio.

SUMMARY

Two dogs and three human subjects were trained in the discrimination of two light windows differing only in intensity of illumination. The chief facts revealed are as follows:

1. With the intensity of the standard light at 1 c.p. and the variable light slightly greater, the least differences at which 30 consecutive errorless discriminations were made by each subject are:

	C. P.	RATIO OF STANDARD TO VARIABLE
Female dog	0.14	8.7 : 10
Male dog	0.2	8.3 : 10
Human H and W	0.11	9.0 : 10
Human S	0.09	9.17 : 10

2. The lowest differences at which clear evidence of discrimination was obtained for each subject are:

	C. P.	PER CENT OF ERRORS	NUMBER OF TRIALS
Female dog	0.12	26.5	75
Male dog	0.10	27.7	90
Human H and W	0.06	20.0	45
Human S	0.04	17.7	45

3. The human subjects were able to discriminate differences lower than those that animals have been trained to discriminate. The dogs discriminated differences lower than those discriminated by the English sparrow (Tugman) and as low as the best record reported (Yerkes) for the discrimination of the dancing mouse.

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REACTIONS OF THE WHITE RAT TO MULTIPLE STIMULI IN TEMPORAL ORDERS

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INTRODUCTION

Maze work with the white rat has so far consisted in having the rat learn to go to some feeding place by means of signals involving spatial relations to a greater or less degree. The rat had to pass through certain alleys and by others until the food was reached. He was thus required to orient himself with respect to the walls and openings or any other cues which he might obtain. Typical of these is the Multiple Choice method as applied to the white rat by Harold E. Burtt.[1] Here the animal was required to guide his running by spatial relations such as the door on the right, the door on the left, or the door next to the right.

PROBLEM

The problem which the experimenters had in mind was to see if the white rat could learn to react to a situation which could be learned only in terms of time (first, second, or third). W. S. Hunter[2] describes an experiment with a temporal maze. His maze resembled a figure eight. At one point the rat must turn either to the right or to the left. Three types of problems were used. The rat was required to make (1) one loop to the left and one to the right, (2) two loops to the left and two to the right, and (3) three loops to the left and three to the right. In the first problem the rat was to learn from the kinæsthetic sensations and from the position of the stops to make the loop on the left first and the one on the right next. All of the rats learned to do this in a short time. In the second problem the rat was forced to make the

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[1] Burtt, Harold E., A study of the behavior of the white rat by the multiple choice method. Jour. Animal Behavior, vi, 1916, 222-246.

[2] Hunter, W. S., The temporal maze and kinaesthetic sensory processes in the white rat. Psychobiology, ii, 1920, 1-17.

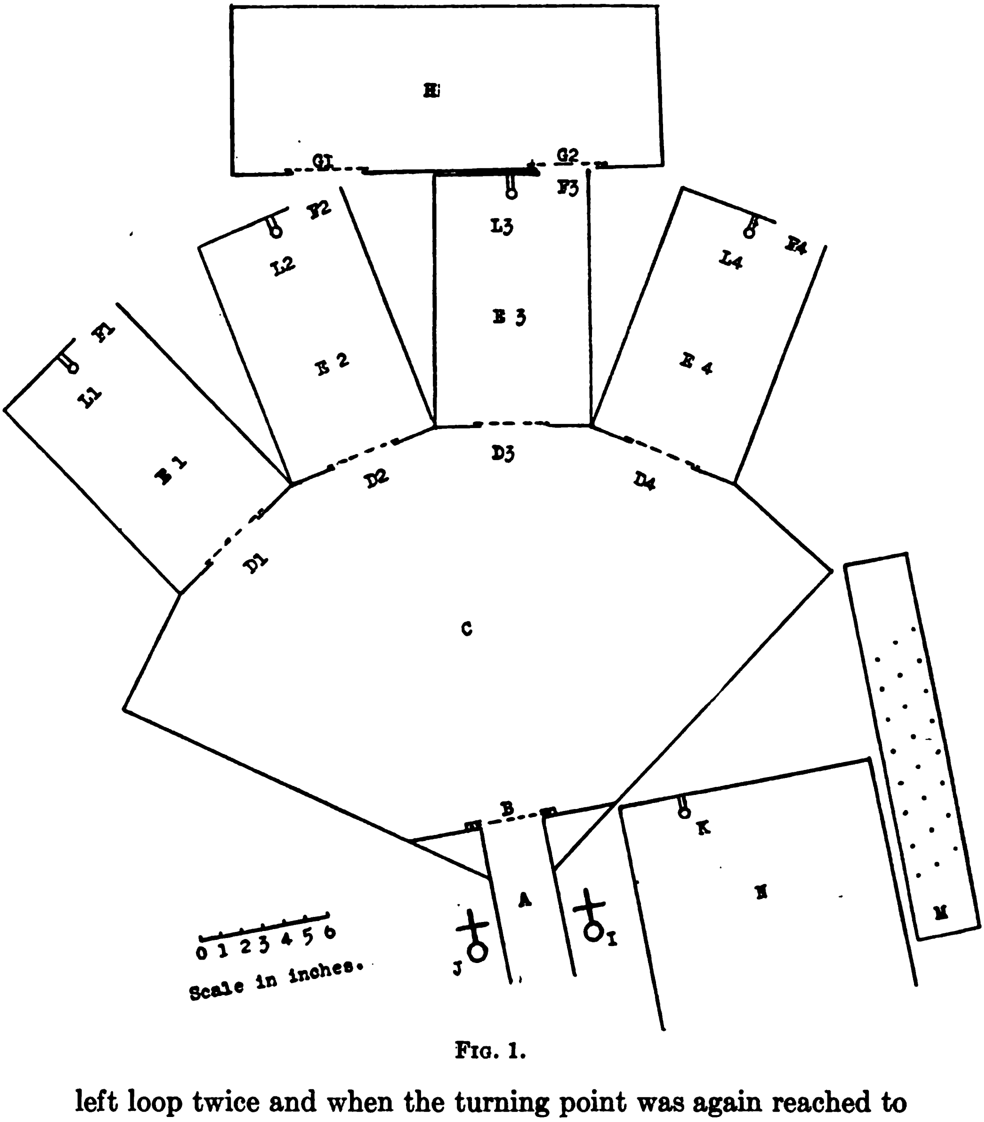


Fig. 1.

left loop twice and when the turning point was again reached to make the right loop twice. He was guided by kinæsthetic sensations and sensations from the stops. This time, however, these sensations meant turn to the left, repeat, and turn to the right. Only one rat solved this problem and that after much coaching. No rat solved the third problem. Hunter concluded that the kinæsthetic sensations may prompt the rat to turn and make the left or the right loop but that the same sensations cannot one time cause the rat to make the same loop over and the next time to turn and make the other loop. Hence his rats failed to learn the temporal maze. In the present experiment the rat was required to go to the first, second, or third of three boxes (out of four) lighted in irregular order.

TABLE 1

Giving description of apparatus

A, entrance alley 7¾ by 3 by 12.

B, glass door 5½ by 8. Door fits into grooves on sides and is raised by means of a string passing through screw eye attached on strip above the door.

C, general runway leading to all doors. Sides 12 high.

DD, glass doors 3 by 4. Can be fastened by means of a strip on inside of *E*. Doors are frosted with white paint. A ½-inch strip along the lower edge of each door is painted black. This was to keep the locking device invisible to the rat.

EE, passage ways 12 by 7¾ by 8.

FF, openings 2½ by 2½ cut into back of *E* to let rat pass to door *G*.

GG, glass doors (transparent) 3 by 4. Free to swing inward only.

H, food box 7½ by 20 by 7½ Has two doors so that it can be fitted to any one of the doors *FF*.

I, telegraph key lighting *K*, which enables the record to be taken while the rat was running in the dark runway *C*.

J, telegraph key lighting a very dim light (about ¼ c.p.). Light was overhead so as to make the rat invisible to experimenter while in the dark.

K, 4-candle power automobile light on 6-volt circuit.

LL, 2-candle power automobile lights on 6-volt circuit.

M, switch board consisting of nails driven through board. Beneath the board wires connect all nails lighting any given light. Circuit was closed by means of a stylus with which the nails were touched.

N, box 11 by 12 by 7 used for recording while the rat was running. Open only on side facing observer.

APPARATUS

Figure 1 (drawn to scale) shows the floor plan of the apparatus used. The following table describes in detail the parts. Numbers refer to inches.

This was built on a table 32 inches from the floor. The walls were 12 inches high. The inside of all boxes and alleys except *H* and *N* was painted black. The method of operation will be explained in a separate section entitled Formal Training.

SUBJECTS

The twelve rats used were obtained from The Wistar Institute at Philadelphia. There were six males and six females. In order to identify them slits were cut in their ears and the hair on their backs was clipped to correspond to the slits. They were numbered and are referred to hereafter by number and sex only. Thus 01m was the number corresponding to the male rat with no slit in the left ear and one in the right ear. For the males and for the females there were the numbers 01, 02, 10, 11, 20, and 21.

While the experiment was not in progress the rats were kept in boxes covered with wire netting. The nest was made of excelsior. Dishes of water were kept in the nests all of the time. The nests were cleaned and disinfected regularly. The food consisted of bread and meat.

From the beginning 02f used the right fore foot very little. This was probably due to an accident in the shipping. This seemed to interfere but little, for she learned in the preliminary training more rapidly than most of the others.

GENERAL METHODS

The experiment was arranged so that three separate problems were attempted at the same time. Three of the rats (01f, 02f, and 10f) were given the problem of going to the first of the lighted doors. All six of the males were given the problem of going to the second of the lighted doors. The three remaining (11f, 20f, and 21f) were given the problem of going to the third or last of the three lighted doors. It was thought that the first and third of the lighted doors would be the ones most easily learned because of the operation of primacy and recency, respectively. For this reason six rats were used on what appeared to be the hardest of the problems. To make the actual operation easier the same doors were lighted for all the rats on any given trial. Three were required to go to the first of the lighted doors, six to the second, and three to the third. The order in which the doors were lighted is shown in table 2. This table also shows the connection of the nails in the switchboard (M) with their respective doors.

Especial care was taken in the making of this arrangement for the lighting of the doors. It will be seen that each door is lighted six times in the eight series. Each door is lighted first twice, second twice, and third twice. Also the doors were arranged so that the door lighted second was one time the left and one time the right of the lighted doors. It was thought that this would aid by giving those who had the hardest problem the benefit of a solution not involving the temporal element solely. This would have been similar to the alternation problem if there had been any signs that they used this method of solution. Also care was taken in the arrangement of the lights so that any door lighted one time as third would the next time be lighted as second. This was done so that the doors would have to be unlocked as few times as possible and hence make the feeding easier. This aided very much in the actual experimenting.

TABLE 2

Arrangement of nails on switchboard and the order in which the doors were lighted

4	2	3
2	3	1
3	1	4
2	4	1
4	1	3
1	3	2
3	2	4
1	4	2

In order to eliminate as far as possible the personal element and make the conditions as uniform as possible the timing of the flashes was done by means of a metronome situated in an adjoining room. The metronome was equipped with mercury contact cups for making and breaking the circuit. This machine was set at the rate of 144 beats per minute during the first 160 trials and at the rate of 192 beats per minute during the remaining 240 trials.

Two feedings were given daily; one in the morning about 8:30 and the other in the evening about 6:30. The feedings lasted usually about an hour each. During this time it was possible to give at first only two trials to each rat. Later it was possible for each to get eight trials during the hour.

PRELIMINARY TRAINING

The preliminary training consisted in first letting the rats pass through the alleys without the glass doors. They were allowed to get their food by passing through any one of the doors. On the third day the doors were attached and the rats allowed to get their food after pushing open the doors. After they had become accustomed to the pushing of the doors, they were taught to go to the lighted door. Each of the doors was then lighted in irregular order. In doing this they did exactly what they later had to do in the formal experiment. They were held in the release box for a few seconds and then allowed to go the lighted door. At the end of 80 trials all of them had learned to go directly to the lighted door. They were then ready for the formal training.

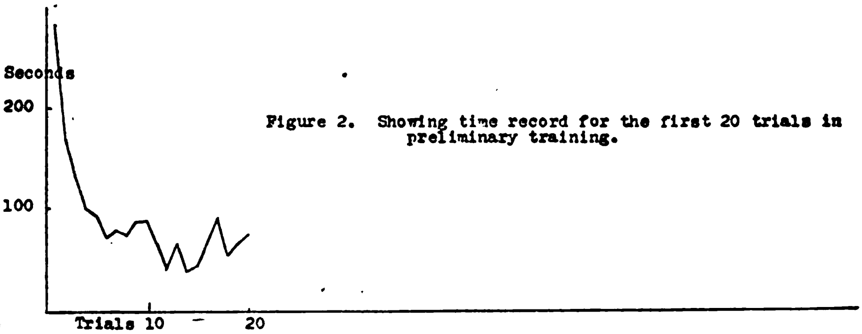


Figure 2. Showing time record for the first 20 trials in preliminary training.

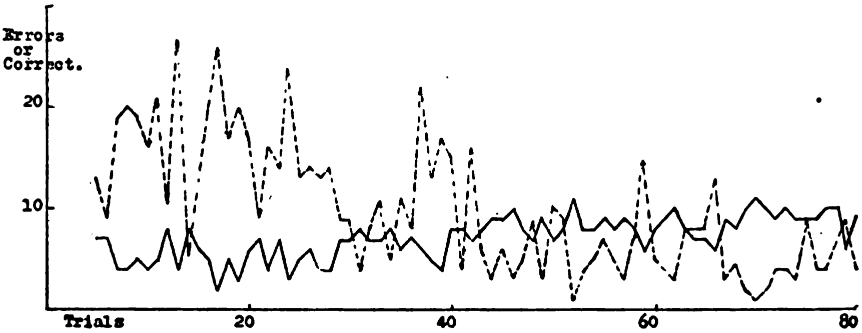


Figure 3. Showing number of errors (broken line) and number of correct responses (solid line) made by the group in preliminary training. No record for first 4 trials.

For the purpose of comparison the time was taken on the first 20 trials and errors were recorded for the entire 80 trials with the exception of the first 4 trials. On these only the time was taken. Here, as throughout the whole experiment, an error consisted in making an attempt to open a wrong door or pushing against a place where there was no door. Figures 2 and 3 show the curves of the preliminary training. Here the errors are taken as the total number of errors made by all the rats in going once to a certain box. The curves for the time and for the number of correct choices are made in the same manner.

It is thus seen that these group curves are all typical learning curves. Some individual differences appeared in the number of trials required for learning. These are not shown, since all of the rats learned the problem before the preliminary training was discontinued.

FORMAL TRAINING

The uniform procedure may best be shown by taking an example. About 6:30 the experimenters darkened the room. The rats were then divided into the four groups shown in table 3. This was done to allow a greater number of trials to be made during any one feeding. The metronome was set at 192. On this particular trial the doors to be lighted were 4, 2, and 3 in the order named. Groups I and II were to go to the second lighted door, which in this case was 2. So this door was unlocked and the food box *H* placed next to the door F2. The rat 01m was then placed in the release box (*A*). While 01m was waiting (all of the rats soon learned to wait with their noses against the glass door *B*) the lights were flashed behind the doors 4, 2, and 3 in the order

TABLE 3

Showing groups of rats and their respective problems

GROUP	INDIVIDUALS			PROBLEM
I	01m,	02m,	10m	Second lighted door
II	11m,	20m,	21m	Second lighted door
III	01f,	02f,	10f	First lighted door
IV	11f,	20f,	21f	Third lighted door

named. The glass door (*B*) was then raised and 01m went to get the food. His errors were noted on blanks in box *N*. Likewise for the other subjects of groups I and II. Door 2 was then locked and door 4 unlocked and the problem thus set for group II, which was then given a trial. Finally door 4 was locked and door 3 unlocked, and the problem thus set for group IV. Each group was allowed 30 seconds in the food box after each trial. In the second trials the doors lighted were 2, 3, and 1 in the order named. The individuals of each group were taken in the order opposite to that followed in the first trials. This was done to equate the time which each rat spent in the food box. On the last trial at each feeding the groups were allowed to remain and get enough food to do them until the next feeding.

Following this same general procedure each rat was given 400 trials. The preliminary training began on February 10 and ended on February 23. The formal training began on February 24 and ended on March 23. Exactly 28 days were required for the formal training.

DISCUSSION OF RESULTS

The accompanying curves (fig. 4) show the results of the formal training. Whatever else these show it is certain that the rats did not learn to go to the proper door for their food. Most of them acquired position habits which they followed more or less constantly until the end of the experiment. The effect of these habits is easily seen. Nearly all of the rats in a series of eight runs made two correct runs. If a rat always followed his position habit, always went to a door, and never repeated any doors, he would make two correct runs and twelve errors in eight successive trials. All of the rats except one (01f) approximated this limit. This accounts for the level which most of the individual curves assume. Only in a few cases did the ratio of correct to wrong responses exceed that of 1:4, which is the ratio to be obtained if position habits only are followed.

Some important results are to be found in the analysis of these position habits. In describing these it will be necessary to take up the record of each rat individually. Table 4 shows the points of first attempts of each rat during intervals of 40 trials, fractions at the top indicating points between doors, "0" and "5" those at left and right ends, respectively.

DESCRIPTION OF POSITION HABITS

In the following description of the habits the path taken will be shown by a series of numbers. Thus 2–1–3–4 would indicate that the rat had the habit of going to 2, turning to the left to 1, returning to 3, and then going to 4 unless the correct door had been found before. Where *R* is used it indicates that the rat returned to the entrance door.

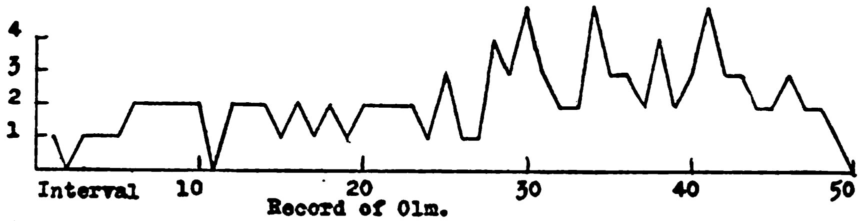
01m early acquired a habit of running to doors 2 or 3, turning to the left, trying doors 2 and 1, turning to the right and trying in order 1, 2, 3, and 4. Typical of these is the form 2–1–3–4. This was frequently modified to 3–2–1–3–4 or 3–1–4–3–2. Whatever the change he always showed the tendency to make the first turn to the left.

02m from the beginning followed the path 4–3–2–1. This was followed to the end with only 48 variations during the 400 trials.

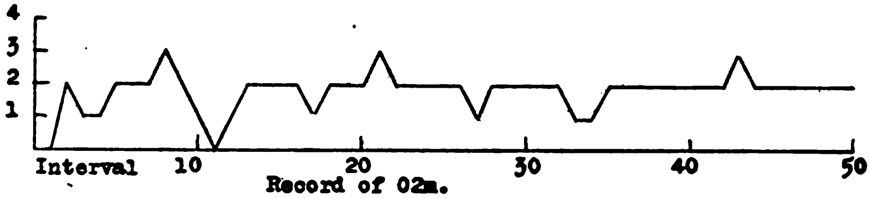
10m first acquired the habit 1–R–4–3–2. This was gradually changed into 1–4–3–2. From the 40th trial to the 80th there was a rather strong tendency to make the run 0–4–3–2–1. The form 1–4–3–2 was then resumed and followed until the end. The method of turning was unchanged. The point of first attempt was the same in 300 of the trials.

11m acquired no very marked habit during the first 40 trials. Doors 2, 3, and 4 were attempted first with about equal frequency. During these trials there was no rather definite tendency to turn to the right or to the left. During the next 120 trials 3 and 4 were attempted first in most of the cases. This rat showed only a very general tendency to try a door to the right and turn to the left. Hence the form was 4–3–2–1 or 3–2–1–2–3–4.

20m started with the habit of 1–R–4–3–2 as the only outstanding habit in the first 20 trials. This was changed to 1–4–3–2 before the 40th trial and was followed steadily until the 160th trial. The form 2–1–4–3 was then followed for about 80 trials.



Record of 01m.



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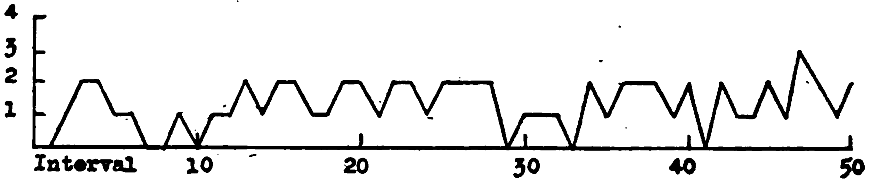

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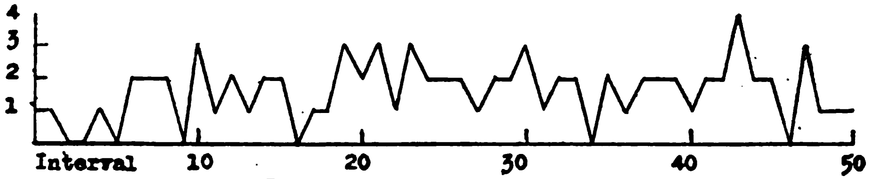

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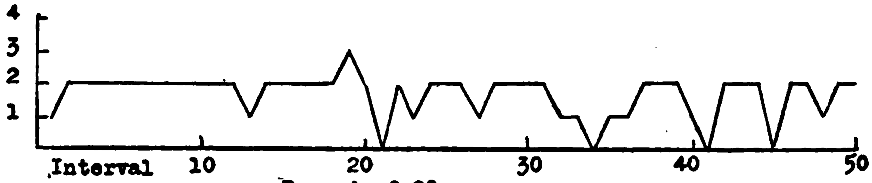
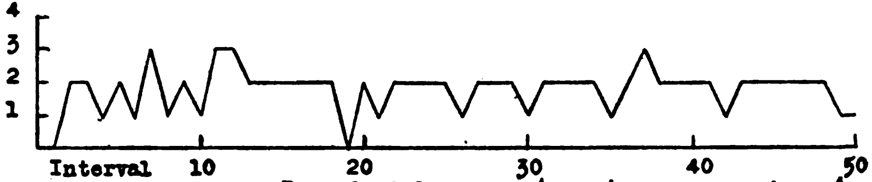
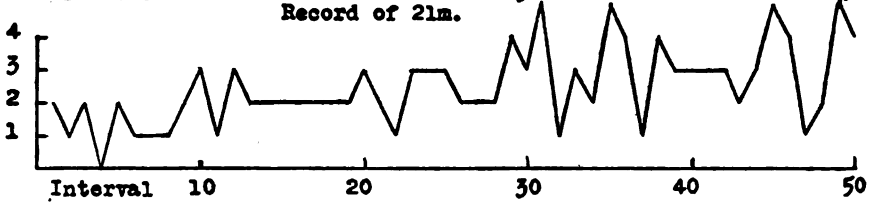


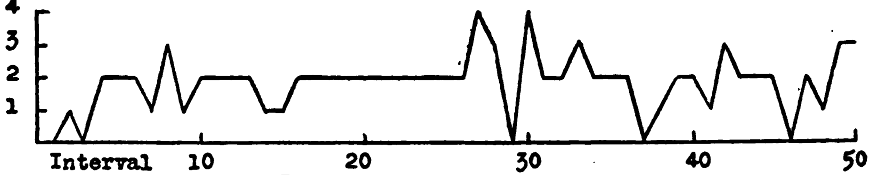
Record of 20m.



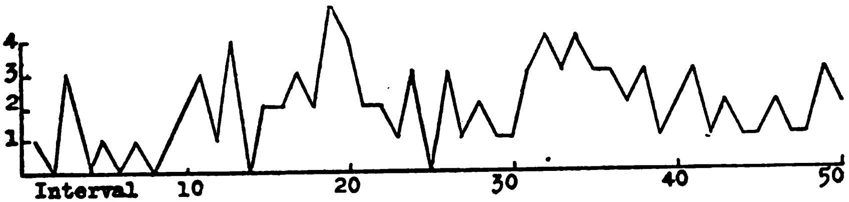
Record of 21m.



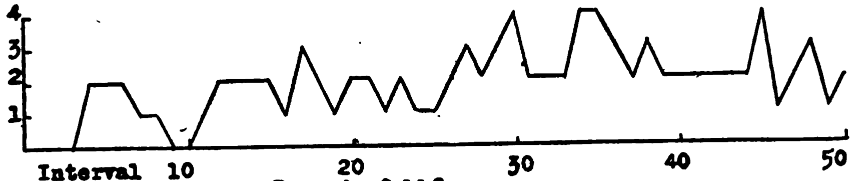
Record of 01f.



Record of 02f.



Record of 10f.



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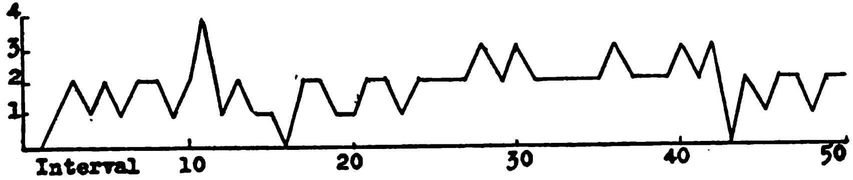
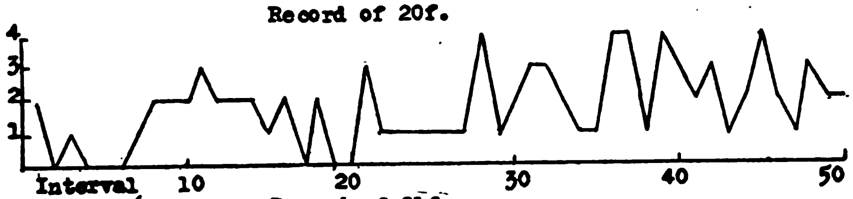


Record of 20f.



Record of 21f.

FIG. 4. INDIVIDUAL CURVES SHOWING NUMBER OF CORRECT RESPONSES DURING INTERVALS OF EIGHT TRIALS

During the next 80 trials both of the above were followed. During the remaining 80 trials the form 2–1–4–3 was resumed. This last was followed almost exclusively (76/80). Here is clearly shown the tendency to keep the same turn but to change the point of first attempt.

21m early showed the tendency to turn to the left after attempting any door. During the first 40 trials 2, 3, and 4 were attempted with about equal frequency. By the 80th trial 4 was attempted most frequently. This was true to the end of the experiment. Only one position habit was formed. That was 4–3–2–1.

TABLE 4

Points of first attempts during intervals of 40 trials

INTERVAL	0	1	1½	2	2½	3	3½	4	5
				Attempts of 01m					
1	0	3	0	11	1	17	3	4	1
2	0	2	0	21	6	8	1	2	0
3	0	3	0	22	9	4	0	2	0
4	0	10	3	18	2	7	0	0	0
5	0	16	1	17	2	2	1	1	0
6	0	11	2	12	0	7	0	8	0
7	0	10	0	13	0	14	0	3	0
8	0	7	1	19	0	12	0	1	0
9	0	6	0	14	0	9	0	11	0
10	1	2	0	11	3	9	1	13	0
Total...	1	70	7	158	23	89	6	45	1
				Attempts of 02m					
1	1	1	0	2	1	6	3	21	5
2	1	1	0	2	0	1	0	35	0
3	0	0	0	0	0	3	2	35	0
4	0	0	0	0	1	2	2	35	0
5	0	0	0	0	0	4	0	36	0
6	0	0	0	0	0	0	0	40	0
7	0	0	0	0	0	5	1	34	0
8	0	0	0	0	0	0	0	40	0
9	0	0	0	0	0	1	0	39	0
10	0	0	0	1	0	2	0	37	0
Total...	2	2	0	5	2	24	8	352	5
				Attempts of 10m					
1	4	23	0	2	0	6	2	3	0
2	21	16	0	2	0	0	0	1	0
3	4	31	1	2	0	1	1	0	0
4	0	34	2	1	0	1	0	2	0
5	2	35	2	0	0	1	0	0	0
6	7	32	0	0	0	1	0	0	0
7	6	31	0	1	0	2	0	0	0
8	2	37	0	1	0	0	0	0	0
9	3	28	3	4	1	0	0	1	0
10	1	33	1	1	1	0	0	3	0
Total...	50	300	9	14	2	12	3	10	0

TABLE 4—*Continued*

INTERVAL	0	1	1½	2	2½	3	3½	4	5
				Attempts of 11m					
1	2	5	0	12	1	10	2	8	0
2	1	0	1	3	1	12	6	14	2
3	0	0	0	0	2	19	3	16	0
4	0	0	0	2	1	12	3	22	0
5	0	0	0	3	0	7	0	30	0
6	0	0	0	3	0	6	0	31	0
7	0	0	0	5	0	10	3	22	0
8	0	1	0	3	0	14	2	20	0
9	0	2	0	5	0	8	3	22	0
10	0	0	0	9	0	10	1	18	2
Total...	3	8	1	45	5	108	23	203	4
				Attempts of 20m					
1	0	35	1	2	0	1	0	1	0
2	0	38	1	1	0	0	0	0	0
3	0	37	2	0	0	1	0	0	0
4	0	29	0	10	0	1	0	0	0
5	0	0	0	32	3	5	0	0	0
6	0	0	0	40	0	0	0	0	0
7	0	0	0	18	1	21	0	0	0
8	0	0	0	16	1	23	0	0	0
9	0	1	0	35	2	2	0	0	0
10	0	3	0	34	1	2	0	0	0
Total...	0	143	4	188	8	56	0	1	0
				Attempts of 21m					
1	2	5	0	9	3	10	1	10	0
2	0	0	0	1	1	14	6	16	2
3	0	1	0	1	1	7	0	30	0
4	0	0	0	2	0	1	2	35	0
5	0	0	0	1	0	1	0	38	0
6	0	0	0	1	0	1	1	37	0
7	0	0	0	0	0	2	1	36	1
8	0	0	0	0	0	1	0	39	0
9	0	0	0	0	0	7	1	32	0
10	2	0	0	1	0	9	1	27	0
Total...	4	6	0	16	5	53	13	300	3

TABLE 4—*Continued*

INTERVAL	0	1	1½	2	2½	3	3½	4	5
Attempts of 01f									
1	1	7	0	7	7	10	1	6	1
2	0	16	3	10	2	8	0	1	0
3	0	2	0	0	1	11	0	25	1
4	0	0	0	1	4	22	4	9	0
5	0	0	0	4	4	16	2	14	0
6	0	2	0	4	0	10	1	23	0
7	0	5	0	12	2	11	2	8	0
8	0	0	1	16	0	13	2	7	0
9	0	3	1	13	3	13	0	7	0
10	0	0	0	12	1	17	1	9	0
Total...	1	35	5	79	25	131	13	109	2
Attempts of 02f									
1	0	1	0	2	1	8	4	20	4
2	0	0	0	0	0	6	1	33	0
3	0	0	0	0	0	0	1	39	0
4	0	0	0	0	0	1	1	38	0
5	0	0	0	0	0	1	0	39	0
6	0	0	0	13	0	14	0	12	0
7	0	0	0	36	1	0	0	3	0
8	0	0	3	21	3	5	0	8	0
9	0	1	0	15	2	11	0	11	0
10	0	0	0	5	3	18	0	14	0
Total...	0	2	3	92	11	64	7	217	4
Attempts of 10f									
1	0	10	1	10	3	6	2	6	2
2	1	2	0	8	5	15	2	7	0
3	0	13	2	6	2	13	2	2	0
4	0	8	0	6	7	16	0	3	0
5	0	13	0	14	4	4	0	5	0
6	0	9	1	22	1	6	0	1	0
7	0	2	0	18	2	13	0	5	0
8	0	4	0	19	4	12	0	1	0
9	0	9	0	18	1	11	0	1	0
10	1	12	1	15	2	8	0	1	0
Total...	2	82	5	136	31	104	6	32	2

TABLE 4—*Concluded*

INTERVAL	0	1	1½	2	2½	3	3½	4	5
Attempts of 11f									
1	4	6	0	7	3	9	2	9	0
2	0	8	1	14	7	10	0	0	0
3	0	2	0	5	4	20	1	8	0
4	0	2	0	5	1	25	6	1	0
5	0	4	0	19	7	9	0	1	0
6	0	0	1	19	0	20	0	0	0
7	0	0	0	23	0	15	0	2	0
8	0	2	0	21	2	14	0	1	0
9	0	7	2	20	2	8	0	1	0
10	1	13	0	16	0	8	0	2	0
Total...	5	44	4	149	26	138	9	25	0
Attempts of 20f									
1	2	22	1	8	1	5	1	0	0
2	0	31	3	2	1	1	0	2	0
3	0	18	7	11	1	2	0	1	0
4	0	5	2	25	3	5	0	0	0
5	0	4	0	34	0	1	0	1	0
6	0	0	0	34	1	5	0	0	0
7	0	0	0	37	2	0	0	1	0
8	1	2	0	33	1	2	0	1	0
9	1	4	3	23	0	5	0	4	0
10	0	1	0	31	0	4	0	2	1
Total...	4	87	16	238	11	30	1	12	1
Attempts of 21f									
1	8	9	2	8	1	8	0	4	0
2	0	27	1	9	0	2	0	1	0
3	0	22	1	14	0	2	0	1	0
4	0	16	3	14	2	2	0	3	0
5	0	13	2	22	1	2	0	0	0
6	0	2	3	24	1	9	0	1	0
7	0	2	3	22	2	8	1	2	0
8	0	1	0	22	0	13	0	4	0
9	0	5	1	25	0	7	0	2	0
10	0	8	0	16	1	14	0	1	0
Total...	8	105	16	176	8	67	1	19	0

01f never fixed definitely any one position habit. During the first 80 trials there was a tendency to turn to the right after trying the door. Afterwards most all of the turns were to the left. Figure 4 shows that this rat continued to improve in the number of correct runs. It seems possible that this rat would have learned its problem with continued practice.

02f during the first 240 trials went most of the time (about 222/240) in the form 4–3–2–1. From the 240th to the 280th trial 2, 3, and 4 were attempted first with about equal frequency. The new habit 2–1–3–4 was then formed. This was followed until about the 320th trial. After that there was no one habit followed. From the 240th to the 320th trial 2 was tried most frequently. During the next 40 trials 2, 3, and 4 were attempted first with about the same frequency. During the last 40 trials 3 and 4 were attempted first with about equal frequency.

10f did not fix any habit. Throughout the entire experiment doors 2, 3, and 4 were attempted first with about equal frequency. This rat turned to the right more than to the left, but turned in each direction frequently. Unlike 01f the curve of this rat does not rise.

11f during the first 80 trials tried 1, 2, and 3 with about equal frequency. A tendency to turn to the right was noticed as early as the 80th trial. This was held to the end. No single form was followed during any part of the experiment. Such forms as 2–3–4–2–1, 3–4, 1–2, and 3–4–2–1 were frequent. Door 3 was attempted first most frequently from the 80th to the 160th trials. For the remainder of the time 2 was attempted first most frequently.

20f early acquired the habit 1–2–3–4. This was followed during the first 120 trials. Then 2–3–4–2–1 and 2–4–3–2–1 were followed almost exclusively. In either case 2 was the first attempted.

21f during the first 40 trials attempted each of the doors with about equal frequency. 65 times during the following 120 trials this rat attempted first 1. 131 of the remaining 240 first attempts were made at 2. No position habit was well formed. A tendency to turn to the right was shown throughout the experiment.

SUMMARY OF STUDY OF POSITION HABITS

A summary of these position habits is difficult to make. This is so because many of the runs were not according to any one type and so many were of mixed types. Three of the rats used the form 4–3–2–1 almost exclusively. About 1000 of their 1200 runs were of this type. The form 1–4–3–2 was used the larger part of the time by two rats. The forms most frequently used were 4–3–2–1, 1–4–3–2, 2–1–4–3, and 1–2–3–4 in the order named.

As far as these results are comparable to those obtained by Hamilton[3] it would seem that the rats used a method which would come between B and C of Hamilton's classification. Type C involved the trying of all doors from right to left or from left to right. Accordingly the above forms 4–3–2–1 and 1–2–3–4 are of type C. Forms 2–1–4–3 and 1–4–3–2 belong either to type B or C.

Forms 2–1–4–3 and 1–4–3–2 cannot be definitely placed because of a vagueness in Hamilton's description of type B. He states that type B involves a trying of all doors in irregular order. Now if by irregular order he means that an animal would one time begin with, say, door 2 and another time with door 4 and in either case try the doors but once each, that is one thing. If he means, however, that an animal tried the doors in, say the order 2–3–4–1 for a long series of trials and then changed to 4–2–3–1 for another series, that is something quite different.

Let us suppose that one subject (X) shows successively the forms 4–1–2–3, 3–2–4–1, 3–4–1–2, 1–2–4–3, etc; and that another subject (Y) shows a repetition of the same form, as 4–1–2–3, 4–1–2–3, 4–1–2–3, etc. Now, by what is apparently Hamilton's method of merely counting the percentage of each irregular-search form in the total reactions of a subject, these two subjects would be rated the same (type B). It is plain, however, that we have here two different levels of random behavior. A subject which tries all four doors regardless of the one which is first attempted certainly shows a higher level of intelligence than one which relies on some position habit repeated identically, even though the position habit may not begin with the door on either end. The behavior of an animal following the former must show clearly the element of trying each of the four doors regardless of the order in which they are taken. That of one following position habits need show merely the following of the habit and only incidentally the trying of all doors. That this is true is seen in the tendency of some animals to go in a position habit even though it may cause them to repeat some of the doors.

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[3] Hamilton, G. V., Trial and error reaction in mammals. Jour. Animal Behavior, i, 1911, 33–67.

Hamilton, G. V., A study of perseverance reactions in primates and rodents. Behavior Monographs, iii, 1916, No. 2.

The whole matter may be summed thus. There is a great deal of difference between the type of response which an animal makes in using several forms of response (without repeating any doors) in irregular temporal order, and that which one makes in using the same position habit (not necessarily beginning with the end) continuously (even though changing the habit to another during a later part of the experiment). Accordingly the forms 2–1–4–3 and 1–4–3–2 would belong somewhere between B and C of Hamilton's classification.

Another matter of interest in the present experiment is the tendency to keep the direction in which the first turn is made after the first attempt at a wrong door. The direction of turn usually remained even though the point of first attempt varied. 11 of the rats kept the direction of turning throughout the experiment. Only one turned in either direction with about equal frequency. 9 made the first turn to the left, 2 made it to the right, and 1 made it either to the left or to the right.

CONCLUSION

The curves in figure 4 show conclusively that the rats did not learn their problems. Only one (01f) continued to improve after the 160th trial. Whether this rat would have continued to improve or would have formed position habits as the others cannot be told.

It is certain, however, that the formation of position habits in an experiment of this sort tends to prevent its solution. No rat following position habits improved after the 160th trial. On the other hand, one of the two not following such habits improved until the end of the experiment. It thus appears that the breaking of position habits, if acquired, is a requisite first step in the solution of this type of problem.

Other experiments have shown that these position habits frequently interfere and sometimes prevent the solution of a problem. Since these habits must be broken the addition of light electric shocks for wrong responses seems a very effective means. Thus not only the reward for the correct responses but also the punishment for the wrong responses would aid in the fixing of the correct form of response.

But why is the tendency toward the formation of position habits so strong? The answer is probably to be found in the study of the animal's native life. Its concern is primarily with objects and their movments. The animal is fitted for such habits as the winding through passages, etc. It is then very natural for the rat in a strange environment to form a type of response which is dependent upon the position of objects in the vicinity.

Since the rat is so skilled in learning problems which involve orientation with respect to things in space, why is it so difficult for it to grasp a temporal order such as that used in this experiment? Here again the answer is to be found in the animal's normal activities. If the perception of time is traceable ultimately to some basis in rate of response used by the subject, the fact that a rat seems to have little use for such delicate adjustment of rates as are used by a stalking animal would lead us to expect small occasion for the development by it of delicate time perceptions. The rat seems to have little use for any adjustment of rates other than a stop, a slow rate, and a rapid rate. These could at best give rise to but a poor sense of temporal relations. For these reasons it does not seem so surprising that the rats failed to show any perception for a complex temporal element as that required for the solution of this problem.

SUGGESTED EXPERIMENT

In attacking the temporal problem Hunter used what may be called the alternation method. As described in the beginning of this paper his rats had to get a cue which would one time serve as a signal to make another turn to the left and on the next loop these same cues were signals to turn to the right. Hence the temporal element must be added in the interpretation given by the subject in reacting. Instead of meaning only one thing, these stimuli under one temporal condition meant one thing and under another temporal condition meant something else.

The method here employed might be called the varied choice method with visual signals. Three doors were lighted in any succession. These lights, instead of being simple stimuli and meaning only a positive response, were complex stimuli and meant that one of them was to be selected in accordance with certain temporal conditions. These temporal conditions constituted the respective problems of the first, second, and third lighted doors.

A third possible method is that employed in experiments with the conditioned reflex. This method may be adapted to either the alternation method or the varied choice method. The cues (temporal at first) can be decreased as the learning progresses and possibly the cues more directly concerned with the temporal phase of the problem may be given more attention. Here again position habits are likely to be formed. If they are formed, light electric shocks may be employed to break them.

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STUDIES OF CEREBRAL FUNCTION IN LEARNING.

II. THE EFFECTS OF LONG CONTINUED PRACTICE UPON CEREBRAL LOCALIZATION

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The automatization of habits through long practice has offered a problem of especial interest to psychologists because of its bearing upon theories of the physiological basis of consciousness, and the literature contains many speculations and dogmatic assertions concerning the changes in localization through which automatic movements are eliminated from consciousness. For the most part these statements are vague in their anatomical reference, as when James ('90) says, "In an habitual action, mere sensation is a sufficient guide, and the upper regions of the brain and mind are set comparatively free." Here we can only infer that by upper regions of the brain the author means the cerebrum, although his application of the term "different heights" to the cerebrum and thalamus makes it fairly certain that this is his meaning. Some other writers have expressed the concept more explicitly. Münsterberg ('00) asserts that "Die Mechanisierung des Übergangs von centripetaler zu centrifugaler Erregung bedeutet die Ausbildung subkortikaler Verbindungen, vermöge deren die von der Peripherie kommende Erschütterung zu Ausführungsbahnen hingeleitet wird, noch ehe sie den psychophysischen Rindenapparat überhaupt erreicht." Similar statements occur frequently in current discussions of automatic movements.

The view seems to rest almost solely upon the conception that cerebral processes are conscious processes and that any activity that ceases to be conscious must necessarily lose its cortical representation. We might disregard such discussions as futile but for the fact that they have led to the general acceptance of the theory that conditioned-reflex arcs, orginally traversing the cortex, may be shortcircuited in the brain stem. The truth or falsity of such an hypothesis is important for an understanding of the physiology of learning, since it involves the concept that the cortex offers greater ease of reintegration of reflex arcs than do subcortical centers, which are, nevertheless, capable of that reintegration. This, in turn, implies that the cortex is in some way specialized for learning, either by reduced resistance to spread of impulses or by some more fundamental physiological change.

The theory of reduction of habits to subcortical levels is, however, based upon very slight evidence and until recently had no sort of factual support. The results of experimental decerebration by Flourens, Schraeder, Goltz, and others give no suggestion of retention of automatized habits and no more crucial experiments were available. In 1902 Franz described the persistence in cats of habits of long standing (coming at call) after operative destruction of the frontal lobes which abolished recently formed habits (latch-box). In later work ('07) he found habits retained in some monkeys and lost in others after frontal lesions and interpreted this as due to the somewhat longer training that the former had received. Since the publication of these results Dr. Franz and I (Lashley and Franz, '17) have found that habits may be retained after partial destruction of the frontal lobes in the rat, when total destruction abolishes them, and I have shown that visual and, hence, probably, auditory habits are not mediated by the frontal lobes (Lashley '20). These data offer an alternative explanation for Franz's earlier results and suggest that they may have been due either to failure to make complete destruction of the frontal lobes or to the differences in the sensory components of the habits which were lost from those which were retained.

In 1916 Dr. Franz and I took up this problem with the rat in an attempt to determine whether long practice in the maze and latch-box would result in loss of cerebral representation. The results of those experiments were not conclusive. The simple maze habit showed no cerebral localization at any stage of learning. The latch-box habit was abolished by total frontal destruction at all stages of training up to three times the amount of practice required for learning, although it survived partial destruction of the frontal pole (Lashley and Franz, '17).

Existing evidence on the reduction of habits to subcortical levels thus seems inconclusive. The clean-cut localization of habits of visual discrimination in the occipital pole of the cerebrum of the rat, which I reported in the first study of this series, offers an excellent opportunity for a final test of the matter. The retention or loss of the habit is easily recognized. The habit involves only a small and well defined area on the occipital pole, and with practice it is soon brought to an accuracy of performance that indicates automatization. Operation in the occipital region produces little shock or interference with habits other than visual. Experiments with vision have thus a decided advantage over those with the kinesthetic-motor habits involved in the frontal pole, for operation in the latter region frequently produces a general deterioration which obscures the specific effects of the injury.

Tests previously reported have shown that destruction of the occipital pole abolished the habit of discrimination between light and dark alleys in the Yerkes discrimination box when the habit had been practiced for about one hundred trials, or until twenty successive errorless trials were obtained. In the present experiments I have tested the effects of the same operation after training had been continued until not less than five hundred successive errorless trials were obtained.

THE VISUAL AREA IN THE RAT

In a previous experiment (Lashley, '20) the entire cerebral cortex was explored by a series of partial destructions in animals which had formed the habit of visual reaction in the problem box. Two additional cases have been obtained with injuries in the temporal and orbital regions, which were imperfectly covered by the first experiments. Figure 1 represents the total extent of the combined lesions in seven animals that retained the habit after operation. The frontal, parietal, orbital, and superior temporal regions are not necessary for the performance of the habit. Figure 2 shows the combined lesions of three animals which lost the habit after operation. The area common to the three is blackened. It seems to represent the only part of the cortex that takes part in the performance of the visual habit.[1]

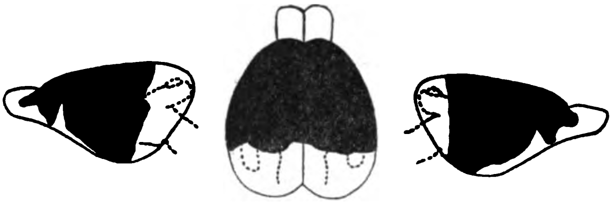

FIG. 1. THE COMBINED EXTENT OF THE LESIONS IN A SERIES OF ANIMALS WHICH SHOWED RETENTION OF THE HABIT OF VISUAL DISCRIMINATION AFTER OPERATION

The lesion in each animal included about one-fourth of the total blackened area shown here.

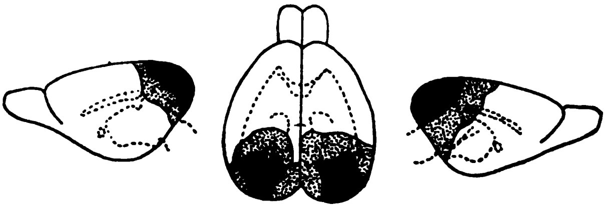

FIG. 2. THE COMBINED EXTENT OF THE LESIONS IN THREE ANIMALS THAT LOST THE HABIT OF VISUAL DISCRIMINATION AFTER OPERATION

The stippled area represents the total extent of the destructions, the black area, that destroyed in all the animals.

METHODS

Training. The animals used in the present experiments were trained in a discrimination box offering a choice of two alleys illuminated and darkened in irregular alternation. Food was given after passage through the illuminated alley and punishment in the darkened. The first three hundred trials were given at the rate of ten per day; thereafter, fifty trials were given daily. Entrance into the dark alley was recorded as an error, but not more than one error per trial was counted. Thus in the records following, errors mean trials in which an error occurred. Training was continued for about fourteen hundred trials.

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[1] The fact that the so-called motor cortex does not participate in the habit has been emphasized in the first paper of the series. Further data on the point will be published later.

Operation. The technique of operation was essentially that described in earlier papers, except that the destructions were made with a thermo-electric cautery inserted cold and heated until the tissues began to boil. This method reduces hemorrhage yet produces a thorough destruction.

Retention tests. The animals were first tested twenty-four hours after operation. Ten trials were given, if possible, and the errors made were recorded, together with notes on the general condition of the animal, its orientation in the problem box, reactions to food, etc. No punishment was given in the first thirty trials of the retention tests, but after these the same methods were used as in the original training and retraining was continued with ten trials per day until discrimination was again established.

When a habit is lost following cerebral injury two interpretations are possible. The operation may have resulted in shock which abolished the habit by producing a general deterioration. In such a case the experiment gives no significant data upon localization of function. Or, the loss may be due to the destruction of the particular conditioned-reflex paths involved in the habit. Practically, it is not easy to distinguish between these two possibilities but observations of the general behavior of the animals usually reveal deterioration when it is present.

Some animals after operation do not orient readily in the prolem box, enter the dark alley many times in a single trial, have difficulty in finding food, climb out of the box repeatedly, and show other signs of confusion or motor disorganization. Such animals usually clear up in from three to five days and show a sudden drop from 50 to zero per cent error in their reactions. Their temporary loss of the habit is clearly due to operative shock.

Other animals show loss of the specific habit without any evidence of general deterioration. They are normally active, oriented in the problem box, correct their errors without perseveration, find the food readily and retain many kinesthetic-motor habits with reference to the problem box. Their relearning is gradual and requires as long as the original learning. The loss in these cases in evidently not due to shock but to some specific effect of the operation upon the visual habit. In general, the occipital cases give little indication of operative shock.[2]

As a further control of operative shock the orbital and temporal regions of one of the animals of the present series were destroyed while the visual area was left intact. The lesions in this animal are more extensive than those of the occipital cases, and approach more closely the nuclei of the optic thalamus. Its retention records are compared with those of the occipital cases.

Reconstruction of lesions. Serial frontal sections of the brains were prepared, stained in iron hematoxylin, and mounted in balsam. Sections at intervals of 0.25 mm. were outlined under the camera lucida at a magnification of ten diameters. The extent of the lesion in each section was determined under higher power and indicated on the outline. The level of the section was next determined and the dimensions of the lesions were transferred to the diagrams of the brain by means of proportional dividers. The points so determined were connected by lines and the resultant areas inked in. Only obvious lesions, absorption of tissue or complete degeneration of the cortex, were recorded, so the diagrams represent the minimal extent of the lesions. The diagrams are probably accurate to within 0.5 mm. when reduced to the dimensions of the brain.

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[2] These statements are based on the records of about 30 animals which have been tested in the visual habit after operation. These cases will be reported later in detail.

EXPERIMENTAL DATA

Four animals were used in these experiments. One (number 13)[3] formed a control in which the orbital surfaces of both hemispheres were destroyed. The others were subjected to occipital injury. The records of the individual cases follow.

Number 13.

Orbital control: Small female about one hundred days old at the beginning of the experiment. Trained in visual discrimination.

Number of trials required for learning: 100.

Number of errors in successive ten trials during learning:

4:5:5:3:1:3:1:0:0:1:0:0:0.

Training was continued for 1400 trials after learning.

Number of errors in successive hundreds of trials in over-training:

3:0:2:0:0:0:0:0:0:0:0:1:0:0.

The orbital surfaces of both hemispheres were cauterized. On the first two days following operation the animal showed disturbances of motor coördination, difficulty in locating the food even when placed in contact with the dish, and lack of orientation in the experiment box. She made numerous errors on these days. On the third day the motor difficulties had cleared up, but errors were still made and there was no indication of retention. On the fourth, fifth, and later days discrimination was perfect.

Post-operative retention tests; errors in successive ten trials: 5:2:6: 0:0:0:0.

Extent of lesions. Figure 3. Right hemisphere. The lesion extends from the level of the knee of the corpus callosum to the level of the anterior margin of the superior colliculus, including all of the orbital and temporal areas within these limits and extending into the parietal and ectorhinal areas. Left hemisphere. The lesion extends from the level of the knee of the corpus callosum to the level of the anterior margin of the superior colliculus, including the orbital and parietal surfaces but not extending so far onto the ectorhinal and temporal surfaces as that on the right. The subcortical nuclei of both sides were ininjured.

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[3] In the first paper of this series visual discrimination was studied in twelve animals. For convenience of reference the animals of the present series are numbered in sequence with those.

This animal, after destruction of the orbital surfaces of the hemispheres, gave evidence of operative shock, but recovered in three days and then showed perfect retention of the visual habit.

Number 14

Small female about one hundred days old at the beginning of the experiment. Trained in visual discrimination.

Number of trials required for learning: 80.

Number of errors in successive ten trials during learning:

6:3:5:4:4:2:2:1:0:0:0.

Training was continued for 1200 trials after learning.

Number of errors in successive hundreds of trials of over-training:

3:1:1:0:0:1:0:0:0:0:0:0.

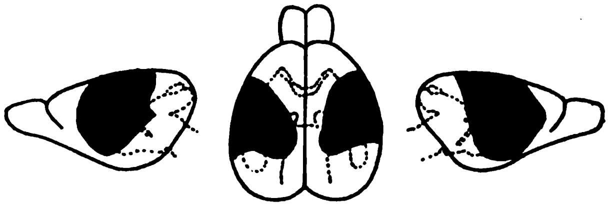

FIG. 3. THE EXTENT OF THE LESIONS IN NUMBER 13

The parietal and orbital surfaces were destroyed

The occipital and parietal lobes were cauterized. The animal was stuporous for five days following the operation and could not be tested. On the sixth day she was active, and when placed in the problem box reacted promptly and seemed well oriented. She never entered the dark alley more than once in a single trial, and, in general, showed retention of the kinesthetic-motor habits. She made many errors, however, and gave no indication of discrimination. Retraining was continued until discrimination appeared. During retraining the animal occasionally gave indication of discrimination, looking back and forth from one alley to the other and choosing correctly, but such reactions were rare, and give only questionable evidence of retention. Her behavior was suggestive of some of the cases of partial destruction of the frontal pole studied earlier, which showed interference with, but not complete loss of the inclined-plane box habit. Such fluctuations in retention after cerebral injury in man have been emphasized by Head

('20). The lesion in this animal probably did not involve all of the visual area.

Post-operative retention tests; errors in successive ten trials:

6:6:5:3:1:4:5:4:3:2:1:0:0:0.

Extent of lesions. Figure 4. Right hemisphere. The lesion begins at the level of the anterior margin of the thalamus and extends caudad to the level of the inferior colliculus, including part of the parietal, occipital and superior temporal regions. It seems to leave the median and extreme caudal portions of the occipital pole intact. Left hemisphere. The lesion begins at the same level as that on the right, and covers approximately the same areas, but does not extend so far over the temporal area and leaves a little more of the occipital pole uninjured. The subcortical nuclei are uninjured.

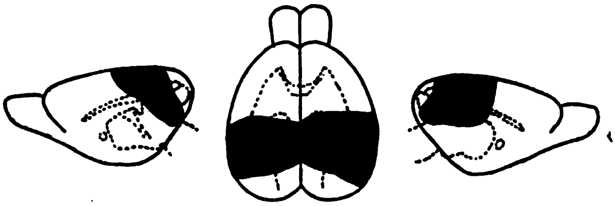

FIG. 4. THE EXTENT OF THE LESIONS IN NUMBER 14

The occipital pole of both hemispheres is uninjured

After injury to the visual area, this animal gave an occasional suggestion of retention but required more practice for reëstablishment of perfect discrimination than was consumed in the original training. The operation probably did not destroy all of the visual area.

Number 15

Small female about one hundred days old at the beginning of the experiment. Trained in visual discrimination.

Number of trials required for learning: 150.

Number of errors in successive ten trials during learning:

7:4:5:5:4:4:7:7:5:4:1:4:3:3:2:0:0:0.

Training was continued for 1200 trials after learning.

Number of errors in successive hundreds of trials of over-training:

1:0:1:0:0:2:0:0:0:0:0:0.

The occipital regions were destroyed by cautery. Retention was first tested twenty-four hours after operation. The animal was active and the box habits were perfectly retained. She corrected her errors promptly, ran directly to the food, scratched at the door of the starting compartment, but gave no indication of retention of the visual habit for 60 trials of the retention tests. In the original training this animal was unusually timid. The fear reactions disappeared during overtraining and did not reappear after operation. The fact that relearning was more rapid than the original learning is probably due to this difference.

Post-operative retention tests; errors in successive ten trials:

7:6:3:6:5:5:2:3:1:0:0.

Extent of lesions. Figure 5. Right hemisphere. The lesion begins at the level of the anterior margin of the superior colliculus and extends caudad to the occipital pole, including all the cortex of the dorsal surface. Left hemisphere. The lesion begins at the same level as that on the right, but does not extend to the pole, an area equal to the thickness of the cortex remaining uninjured in the caudal region. The subcortical nuclei are uninjured.

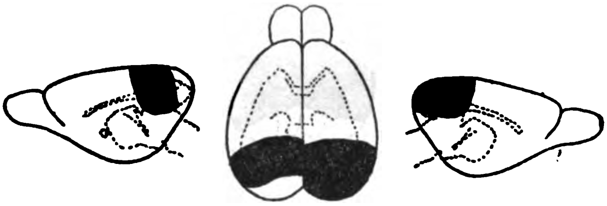

FIG. 5. THE EXTENT OF THE LESIONS IN NUMBER 15

After destruction of the visual cortex this animal showed no evidence of shock, but failed to retain the visual habit. The habit was regained with less practice than was required for the original learning, owing, probably, to the retention of kinesthetic-motor habits and reduction of timidity.

Number 16

Small female about one hundred days old at the beginning of the experiment. Trained in visual discrimination.

Number of trials required for learning: 100.

Number of errors in successive ten trials during learning:

6:7:7:2:3:1:1:1:0:1:0:0:0.

Training was continued for 1200 trials after learning.

Number of errors in successive hundreds of trials of overtraining:

4:0:0:0:0:1:0:0:0:0:0:0.

The occipital cortex was destroyed by cautery. Retention was first tested twenty-four hours after operation. The animal was very active and seemed quite normal in her reactions. She corrected errors promptly and never hesitated on the way to the feeding compartment. She gave no evidence of retention of the visual habit on this or the following days.

Post-operative retention tests; errors in successive ten trials:

7:4:3:4:5:5:3:3:4:1:1:0:0.

Extent of lesions. Figure 6. Right hemisphere. The lesion extends from the level of the anterior margin of the nucleus habenulae to the posterior limit of the external capsule. All the cortex of the dorsal surface within these limits is destroyed. The cortex of the inferior temporal area and of the extreme occipital pole is intact. Left hemisphere. The lesion begins at the level of the pillars of the fornix and includes approximately the same area as that on the right, but does not extend onto the temporal surface. The dorsal lobes of the hippocampus are destroyed on both sides. The subcortical nuclei are uninjured.

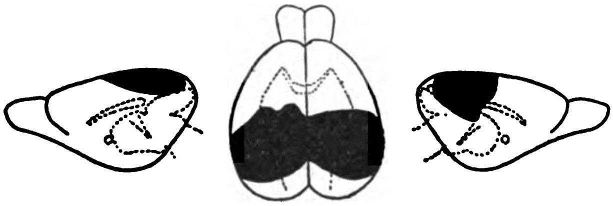

FIG. 6. THE EXTENT OF THE LESIONS IN NUMBER 16

After destruction of the visual area, this animal showed normal activity, retention oj kinesthetic-motor habits, and complete loss of the visual habit. Relearning required a little more practice than learning.

The results of these tests are summarized in table 1. The control animal (number 13), with extensive lesions in the orbital regions, showed confusion for three days, then quick recovery with immediate reappearance of the habit. Number 14, after occipital injury, was stuporous for five days, then regained normal acitvity and kinesthetic-motor habits, but did not make a perfect record until after retraining for 110 trials. Numbers 15 and 16, after occipital lesions, gave no indication of shock from the operation, retained kinesthetic-motor habits, but lost the visual habit.

These animals had all received 1200 or more trials of over-training. The records of numbers 10 and 11 from the earlier experiments are included in table 1. The occipital lobes of these animals were destroyed after less than one hundred trials in the discrimination box. Both show loss of the habit, with relearning as rapid as that of the over-trained animals. The over-training, therefore, does not increase the power of the habit to survive occipital injury or to be reacquired after the operation.

TABLE 1

Errors made in retention tests by animals after operation. Five errors in ten trials is the chance expectation and indicates lack of discrimination. Number 13, temporal lesions; numbers 14, 15, and 16, occipital lesions after long training; numbers 10 and 11, occipital lesions after brief training.

NUMBER	TRIALS FOR LEARNING	TRIALS FOR RELEARNING	ERRORS IN SUCCESSIVE TEN TRIALS OF RETENTION TESTS
13	100	30	5 2 6 0 0 0
14	80	110	6 6 5 3 1 4 5 4 3 2 1 0 0 0
15	110	90	7 6 3 6 5 5 2 3 1 0 0
16	100	110	7 4 3 4 5 5 3 3 4 1 1 0 0
10	60	100	6 5 5 3 3 0 1 0 2 2 0 0
11	60	74	3 (of 4) 6 4 9 2 0 3 4 0 0

Early in the retention tests all of the over-trained animals gave evidence of normal activity and retention of the kinesthetic-motor habits of the discrimination box. The loss of the habit in them, therefore, can not be ascribed to general shock resulting from the operation. The direct effect of the operation upon lower visual centers is also ruled out by the facts, first, that these centers showed no microscopically detectable lesions and, second, by the results with number 13, in which the cautery passed close to the pulvinar and external geniculate body on both sides, without abolishing the habit. From this it seems clear that the loss of the habit is not due to shock or to injury to the optic thalamus but is a specific effect of the destruction of the visual cortex.

These considerations lead to the conclusion that overtraining up to twelve hundred trials does not alter the cerebral localization of the habit of visual discrimination. The functional activity of the visual cortex is still necessary to the performance of the habit and there is no indication that subcortical nuclei have taken over any part of the reaction, even sufficient to facilitate relearning. The cerebral area functional in learning seems to retain the same function after prolonged training.

THE QUESTION OF AUTOMATIZATION

Thus far in the discussion I have taken it for granted that 1300 trials of practice would produce automatization. That assumption, however, is open to question. The test for automatization in man is the ability to carry on the habitual act while the verbal or other complex mechanisms are making other non-habitual adjustments. In the rat such a test is not readily practicable and it is necessary to fall back upon indirect methods of judging the degree of mechanization of the habit.

No data upon the amount of practice necessary to automatize a simple visuo-motor habit in man are available, but certainly less than 1000 trials are sometimes effective. Some of my own automatic reactions, such as changing to a laboratory coat, have been established in less than 500 trials. The habits of shifting the stimulus lights and doors of the discrimination box are automatized by the experimenter long before the animals cease to make errors in discrimination.

It can not be argued, then, that 1300 trials are inadequate to produce mechanization under some conditions. Whether they did so in these experiments can be judged only from general observations on the behavior of the animals. In the early stages of training the rat frequently hesitates at the entrance to the alleys, sways back and forth as though looking at first one, then the other stimulus plate, and finally advances slowly into one alley. Later, these comparing movements disappear, but for several hundred trials they occasionally recur. At this stage of learning, if the animal is disturbed by a noise or jar as it enters the alley, it stops, makes comparing movements or smells at the stimulus plate, and explores the alleys before passing to the food compartment. By the end of the thousandth trial all such reactions had disappeared in these animals. The door of the starting compartment is of glass so that the stimulus plates are visible through it. Seemingly the animals, after a thousand trials, were oriented toward the light before the door was opened and dashed through to the food the moment the door was raised. If they were distracted on the way, they did not stop and make comparing movements, but whirled about and returned to the starting compartment, then rushed back through the illuminated alley. This last bit of behavior is highly suggestive of the human tendency to start an automatized reaction all over again when it is interrupted, and forms the strongest evidence available that the habit was really mechanized. Finally, the regularity and accuracy with which the habit appeared and the fact that none of the animals made more than three errors in the last thousand trials indicate that the reaction was reduced to a condition where it required no very complex adjustments. From these facts it seems almost certain that, after a thousand trials, the visual habit had reached a stage comparable to that of complete automatization in man.

THE NEUROLOGICAL BASIS OF AUTOMATIZATION

The evidence from pathological cases in man is inconclusive. Occasionally a long practiced habit, such as the patient's ability to recall his name, persists in an otherwise complete aphasia. But a strong case could be made for the view that these habits persist, not by virtue of long practice, but because they have a deep emotional significance and hence receive a tonic reinforcement that is lacking to other activities. Certainly, hymn singing and profanity are among the most common accomplishments of aphasiacs. On the other hand, many activities that are certainly automatized in the great majority of individuals fail to survive in apraxia or aphasia from cerebral lesions and so are clearly not mediated by subcortical nuclei. The pathological evidence certainly can not be advanced as proof of the reduction of habits to subcortical levels.

Many, perhaps the majority of automatized habits in man, have a proprioceptive rather than exteroceptive basis. The present experiments seem adequate to prove that visual habits are not reduced to subcortical levels by long practice. Is the same true of habits having a kinesthetic basis? The work of Franz and the writer indicated that the latch-box habit was abolished by frontal destruction after a considerable period of overtraining. Simpler habits, such as that of the maze with a single *cul de sac*, were undisturbed by frontal lesions even after only a brief period of training. I have since tested retention of the discrimination box habits after extensive lesions involving, in one or another animal, every part of the cerebrum except the ectorhinal area. Irrespective of the position of the lesions these animals have shown fairly perfect retention of the kinesthetic-motor habits of the discrimination box. The simple tactual- and kinesthetic-motor habits have no cerebral localization at any stage of training. Whether they are mediated by diffuse arcs passing through all parts of the cortex, or are formed at subcortical levels is uncertain. I have obtained formation of such habits in nearly decerebrate animals, in which only the inferior temporal and ectorhinal areas were intact, but this is not sufficient to settle the question.

Whatever the explanation of these facts, they suggest that for kinesthetic-motor as well as for visual habits, the cortical or subcortical representation is determined at the time of learning and is not modified by subsequent practice.

If long practiced habits are not reduced to subcortical levels, what is the neurological basis of automatization? The musician may not speak when first learning a difficult movement but later his verbal reactions are dissociated from the manual coördinations so that the two processes may go on simultaneously. It is this capactiy to function without exciting reaction systems other than those directly concerned with its performance that characterizes the automatic habit. Such a condition might be brought about by blocking cerebral associative connections, and this seems to be the only alternative to reduction to subcortical levels. An analogous situation is presented by the differentiation of the conditioned reflex to a specific stimulus (Bechterew, '13). Whether the confining of impulses to a single path is the result merely of repetition or of some active inhibitory or blocking process can not be decided from existing evidence. Some professional musicians who have never practiced talking while playing do not show the dissociation of the functions, and I have seen the dissociation increased by practice. This would indicate that some active conflict between the reactions is essential to their dissociation, and suggests that, perhaps, the mechanisms of automatization and of dissociation through conflict and repression in the Freudian sense may have much in common.

SUMMARY

Animals were trained in a habit of visual discrimination and were then given additional practice for twelve hundred trials. Destruction of the visual area after this training resulted in loss of the habit. Retention of other habits showed that the loss was due to destruction of visual conditioned-reflex arcs and not to operative shock. Long training did not reduce the habit to subcortical levels.

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THE RELATION OF THE LATERAL LINE ORGANS OF NECTURUS TO HEARING

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Among senses hearing is quite unusual. It is restricted to comparatively few groups of animals, and those possessing it are highly organized members of their groups. The Arthropods and vertebrates seem to be the only animals that can really hear; all others probably live in silence. The ear is an unusually perfect mechanism, even excelling the eye. An untrained human ear is able to distinguish between two musical tones struck simultaneously, but the eye is not able to distinguish the various colors that make up "white" light and is easily deceived by blends and mixtures.

The ears of aquatic vertebrates are on the whole less efficient than those of birds and mammals. However, fishes, amphibian tadpoles, and adult aquatic salamanders have accessory organs for perceiving vibratory stimuli which the more specialized vertebrates lack. These are the lateral line organs. In fishes the auditory apparatus is rather simple, consisting of an "inner" ear, imbedded in the skull, and the lateral line organs. Parker (1903–1911) has demonstrated that a fish's ear is sensitive to sounds and that the lateral line also serves as an accessory organ of hearing, responding to vibrations of low rate.

Though aquatic amphibians possess lateral line organs, their reactions to sounds have not been studied. This paper describes experiments which were carried out in order to test the function of the lateral line organs of the mudpuppy in relation to slow vibrations in water.

Following the methods used by Parker (1905), a wire cage was suspended from the ceiling so that it hung partly in the water of a large aquarium. On one side of the aquarium a wire was stretched so that it could be made to vibrate at a low rate—120 times or less per second. There was no opportunity for vibrations which originated on the floor or sides of the aquarium to be communicated to a mudpuppy in the cage, except through water.

Two types of mudpuppies were tested: (1) normal individuals and (2) those in which the nerves supplying the lateral line organs had been cut. After preliminary dissections and experiments it was found that the seventh and tenth cranial nerves could be readily cut by making a small incision just behind the gills.

When a normal mudpuppy was placed in the cage and the wire was set in vibration, three chief types of responses were observed: (1) the rate of respiratory movements of the gills increased and was maintained at a higher level for a brief period; (2) the gills, if moving, stopped; (3) the whole animal might make a sudden movement, as if startled by the sound.

The most satisfactory criterion in observing the behavior of the animals was the rate of gill movements, and this was used as a standard throughout the experiments. Ten normal mudpuppies were each subjected to ten tests in the apparatus and the responses to the vibration of the wire were made ninety-five times. When five mudpuppies in which the nerve connections to the lateral line system had been severed were subjected to similar tests only three doubtful responses resulted in fifty trials. These results indicate that the lateral line of the mudpuppy serves the same function in aquatic amphibians that it does in fishes.

Observations were made which appear to indicate the length of time required for the reestablishment of connections with the lateral line organs after the nerves had been cut. The animals in which the lateral line nerves had been cut were subjected to the tests at intervals of two days. About the eighteenth day from the time of the operation, doubtful responses were noticed. On the twenty-first day the animals responded as they did before the operation. This indicates that the regeneration of the connections had taken place and also gives further support to the view that the lateral line system is of importance in the discrimination of sound vibrations.

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COÖRDINATION IN THE STARFISH

I. BEHAVIOR OF THE INDIVIDUAL TUBE FEET

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It is the object of this paper to inquire in some detail into the behavior and reflexes of the individual tube feet of certain starfish and to describe some experiments designed to add to our knowledge of the physiology of these organs. I hope later to publish papers on the locomotor and righting activities of starfish under normal and various unusual conditions.

The work was prosecuted under the direction and with the kindly help and criticism of Professor S. J. Holmes of the Department of Zoology, University of California. I also received valuable advice from Professors S. S. Maxwell and T. C. Burnett of the Physiology Department, University of California. Professor W. K. Fisher of the Hopkins Marine Station of Stanford University extended to me every courtesy and excellent laboratory facilities during my short sojourn in Pacific Grove. He determined and helped me collect my material. I wish to express my sincere thanks to these men.

To my wife, for her many cheerful sacrifices and her willing help in numerous ways, is due my fullest gratitude.

LITERATURE

Reamur (1710) was perhaps the first to analyze adequately the mechanism of extension and retraction of the tube feet. In his description we find the first expression of an ancient zoölogical tradition, attributing locomotion in the starfish to the shortening of tube feet which have been thrust out in front and attached to the substrate. Romanes and Ewart (1881), Preyer (1886), Mangold (1908) are some of the physiologists who record the activities of the tube feet from a purely descriptive viewpoint subordinating these observations to descriptions of the behavior of the animal as a whole. Mangold (1908a), however, has worked out in some detail the relation between the activity of the ampullae and tube feet and has sketched the various postures assumed by the latter organs in locomotion and in the self-burying reaction (*Astropecten*).

The work of Jennings (1907) was also descriptive in nature but marked an advance in that it showed the tube feet serving as "legs" for pushing rather than as "ropes" for hauling the animal forward. According to Jennings "They (the feet) do not pull, but on the contrary they push backward a certain amount just as do the feet of a higher animal." The suckers merely prevent the tube feet from slipping. The tube feet function as more or less rigid supports to the animal elevating it, during locomotion, well above the substrate. This has been confirmed by Cole (1913a).

PHYSIOLOGICAL STATES

Before beginning an analysis of the physiology of the tube feet, I shall describe three different physiological states, since the behavior of the tube feet (and other organs) differs markedly under the different physiological conditions.

Starfish are spoken of as locomotor when all of the tube feet are "oriented" in some one direction. The tube feet reach out actively toward the temporary anterior and may or may not be stepping, depending upon conditions which will be considered later.

Starfish are spoken of as active but unoriented when the tube feet of the different rays are active in a locomotor sense but there is no coördination in the direction of their activity. The tube feet are usually oriented toward the tip of the ray on which they are situated.

Starfish are spoken of as rigid when the tube feet show a strong tendency to attach and the muscles of the myodermal sheath are so contracted as to make the animal extremely stiff and sluggish. There is little or no tendency toward locomotion, the tube feet often being oriented somewhat toward the disk.

The physiological states merge gradually into one another, though in their typical state they are clearly marked. They have been noted in the following species: *Pisaster ocraceus* (Brandt), *Pycnopodia helianthoides* (Stimpson) and *Asterina miniata* (Brandt).

On the surf-beaten rocks of Monterey bay, near the Hopkins marine station, *Pisaster ocraceus* is always in the rigid attached state and will often remain so even after being kept in an aquarium for as much as three weeks, after which time it may start active migration. This is not true of other species, and the same species when collected from the quiet waters of Oakland harbor begins migration as soon as it is placed in the aquarium. This difference in behavior is probably a habitat response and its persistence for as much as three weeks is a rather striking example of what may be called the physiological inertia of the animal.

REACTIONS OF THE TUBE-FEET

The tube feet in the course of their normal activities are seen to extend, to retract, to attach and to perform the locomotor step reflex. In the following pages we shall inquire into the physiological relationship of these reactions to each other, to the physiological state of the organism and to various types of stimulation.

Extension

Extension of the tube feet, as is well known, involves a contraction of the ampullar muscles and a relaxation of the longitudinal muscles of the tube foot.

Extension is spontaneous in animals of any of the three physiological states, though often least active in rigid starfish. In locomotor, or active but unoriented animals the tube feet extend at an angle to the arm which, as will be shown, is related to the direction of locomotion.

In order to learn how dependent extension is upon the central nervous system, tube feet were amputated and equipped with "artificial ampullae," by tying them over the end of a capillary tube (fig. 1) which was connected through a long rubber tube with a reservoir of sea water. By raising or lowering this reservoir, the pressure against the inside of the tube foot could be varied. When under 10 cm. (H_2O) pressure, the tube foot would extend, though much more slowly than normally, and not to its complete length. Increasing the pressure to 2 m. (H_2O) increased the extent but not noticeably the rate of elongation. Tube foot preparations, if anesthetized in $MgSO_4$ presented an increase in length and diameter showing a relaxation of their longitudinal and circular muscles. The injected tube foot preparation, will if bent to one side, remain in that position for a few seconds and slowly resume its original curvature. (Cf. Von Uexküll (1900) on the reactions of sea urchin spines.) These movements are physiological because they do not persist after anesthetization. Tube foot preparations all were curved toward the pseudohemal canal (Cuenot, 1888). This curvature persists in the anesthetized or dead preparations and is therefore probably due to mechanical rather than to physiological causes.

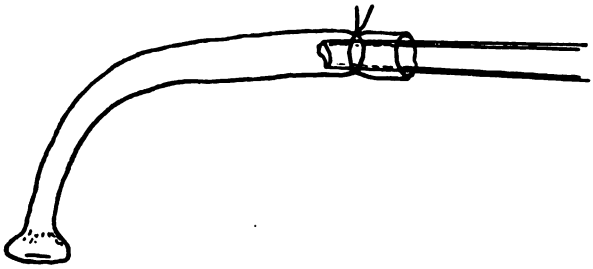

Fig. 1. An Isolated Tube Foot Preparation

Attachment

Attaching is conditioned by the physiological state of the animal. Rigid animals are much more likely to attach strongly than animals in either of the other physiological states. The reflex is an incupping at the center of a properly stimulated area of the disk, no matter what the size or shape of this area may be. This is illustrated by gently touching the disk with a blunt needle-point or the side of a small hook. The tube foot will attach to these objects so strongly that it may be pulled quite out of shape (fig. 2) before releasing.

Tube foot preparations, made very carefully, were found, upon being placed against a substrate, to attach and hold against enough tension to sometimes tear off a part of the disk (fig. 3). This power of attaching was lost after a few trials.

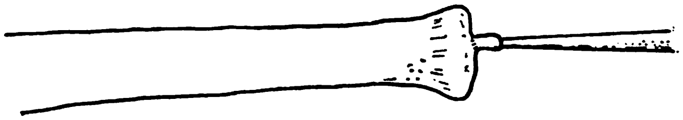

Fig. 2. A Tube Foot Attached to a Blunt Needle Point Which has been Pulled so Strongly as to Distort the Disk

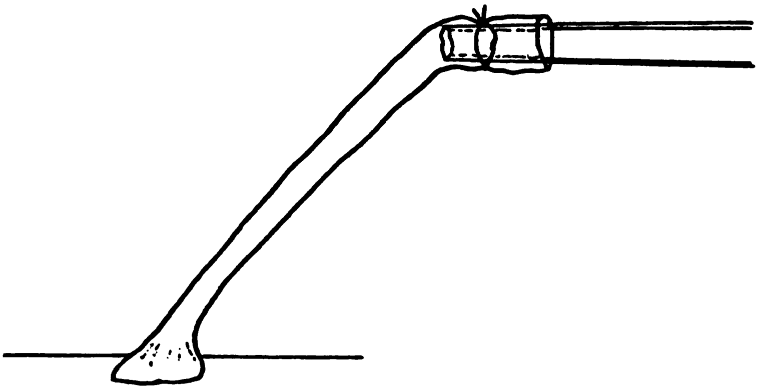

Fig. 3. An Isolated Tube Foot Preparation Attaching to a Smooth Substrate

It was noted with considerable interest that tube foot preparations made from rigid starfish were much more likely to attach than those made from animals in active locomotion, or that were feeling about the surface film. Thus a tube foot may attach when not connected with the central nervous system (see Von Uexküll, 1900, Cowles, 1911, Clark, 1899, Bottazzi, 1898 and Russo, 1913) and fluctuations in the physiological tendencies of the organism may persist in its amputated organs (see Von Uexküll 1903 on the trapping of "tonus").

Withdrawal and detachment

Harsh contact stimulation of any part of the pedicel will usually induce both withdrawal and detachment, as will gentle stimulation of the side of the column. Gentle stimulation of the disk, however, often induces withdrawal without detachment as can be shown by touching the disk with a light suspended object (fig. 4). This reaction occurs when the rigid starfish shrinks down tightly against the substrate, and as the rigid state merges into the locomotor state, this type of withdrawal intergrades with the step reflex.

Detachment and shortening of the longitudinal musculature follow harsh stimulation of the attached isolated tube foot preparation.

The step reflex

The step reflex, which is concerned in ordinary locomotion and in the righting reaction, is initiated only by contact stimulation of the disk of tube feet which are in the locomotor state. If an animal which is in the rigid state be stimulated to crawl, it will gradually change into the locomotor state. Tube feet, stimulated on the disk during this change present a series of reactions (figs. 4, 5 and 6) which show every gradation from simple withdrawal to a complete and normal step reflex. In this transformation, the first change involves an extension of the tube foot immediately after the stimulation (by some light suspended object) and a slight movement of the disk away from the direction of extension before the retraction (fig. 5). As the rigid state passes off the extension and bending back become more pronounced resulting finally in the normal step reflex (fig. 6). The result of the extension is the support of the animal by the rigid tube feet acting as legs (see fig. 7, after Jennings, 1907). The result of the bending back of the tube foot is the forward movement of the starfish.

Under the ordinary circumstances of locomotion the moving back of the tube foot is followed by retraction, and retraction by reextension in the direction of locomotion. In case this reextension involves contact with the substrate the step reflex is

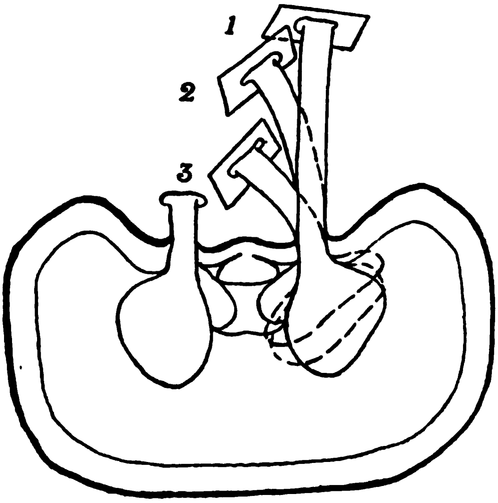

FIG. 4. DIAGRAM OF THE WITHDRAWING OF A TUBE FOOT ON A RIGID STARFISH
The disk has been stimulated by a light object to which it has attached

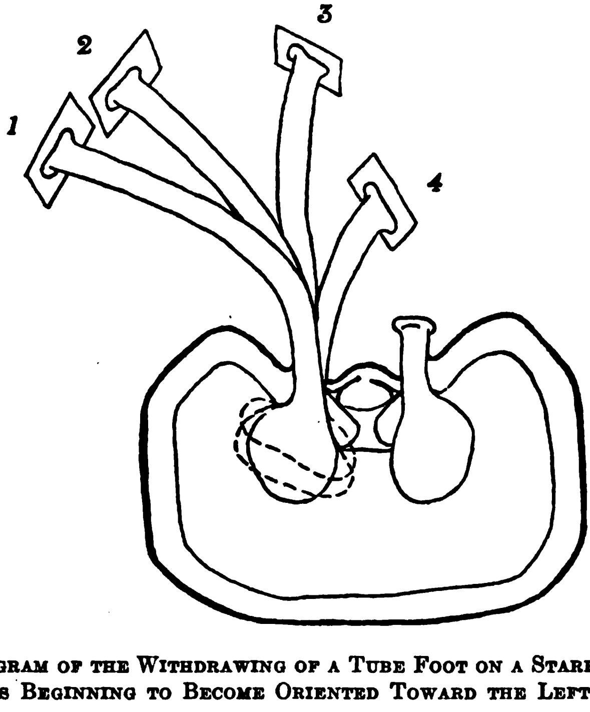

FIG. 5. DIAGRAM OF THE WITHDRAWING OF A TUBE FOOT ON A STARFISH WHICH IS BEGINNING TO BECOME ORIENTED TOWARD THE LEFT
The disk has been stimulated by a light object to which it has attached but instead of a simply withdrawing it extends somewhat and sweeps back to the right.

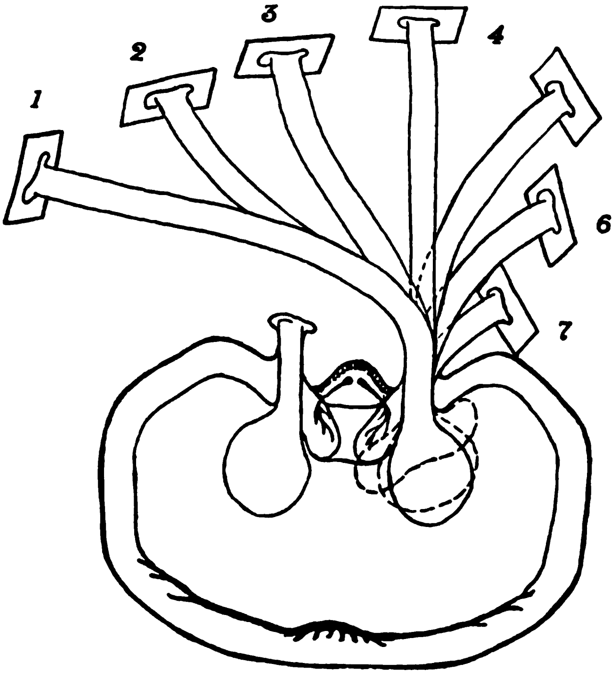

FIG. 6. DIAGRAM OF THE STEP REFLEX AS A RESPONSE TO STIMULATION BY A LIGHT OBJECT

Note similarity to withdrawing response (fig. 5). (Nerve elements as illustrated by Cuenot, 1888.)

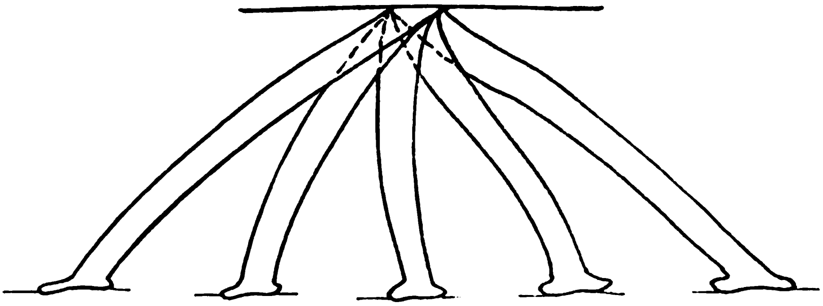

FIG. 7. DIAGRAM SHOWING HOW THE TUBE FEET, ACTING AS LEGS, KEEP THE ANIMAL WELL ABOVE THE SUBSTRATE (AFTER JENNINGS, 1907)

repeated. These movements which involve attachment to the substrate, as will be shown in detail later, are those of ordinary locomotion. As long as the locomotor impluse remains unimpaired and contact stimuli result from extension, each tube foot repeats the step reflex in harmony with its fellows as to direction but independently as to time.

The necessity of the contact stimulation for the initiation of the step reflex is made clear by the following experiment: A large specimen of *Pycnopodia* was inverted under water while migrating. The tube feet extended themselves toward the temporary anterior and remained so extended without any sign of the step reflex. A piece of thin celluloid was then laid upon the tube feet. The contact stimulus thus provided elicited immediately vigorous step reflexes, so that the celluloid was rapidly "walked" to the temporary posterior of the starfish. Tube feet which came in contact with the surface film of the water also underwent the step reflex. It was thus found that animals held ventral side up in the air executed active step

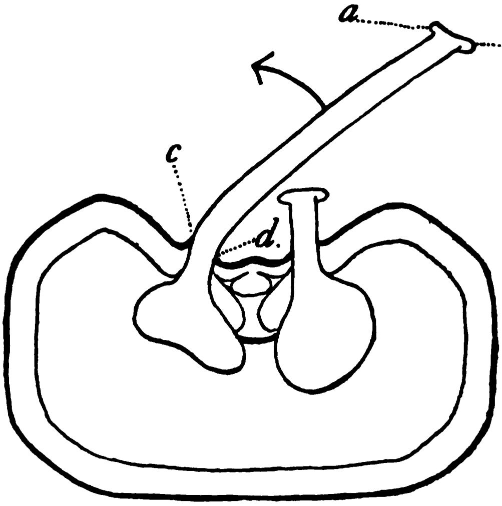

Fig. 8. Diagram Illustrating Differential Sensibility in the Oriented Tube Foot

A normal step reflex is more apt to follow contact stimulation at *a* than at *b* reflexes with all their tube feet, the contact stimulus being furnished by the surface film contracting around the tube foot.

Upon what does the direction of the back sweep depend? If an extended oriented tube foot (fig. 8) be stimulated at the point b, it is less apt to give the typical step reflex than if stimulated at a. There is therefore some differential sensibility of the various parts of the ambulacral disk. In ordinary locomotion, however, the disk being pressed flat against the substrate, and in case of the step reflex elicited from the contact of the surface film, the whole of the disk is stimulated. The orientation of the step reflex is, therefore, under ordinary circumstances, not dependent upon the location of the stimulus so much as upon the greater excitability of the muscles on the distal side of the tube foot (*a-c*, fig. 8). This is comparable to the increased tension of the muscles *b d* in the precontact stages of the step reflex (see Von Uexküll, 1903).

We can best leave the analysis of oriented coördination for later consideration and say at this place that the orientation of the step reflex takes the direction of the orientation of that part of the animal under observation.

Status of the attaching reflex during the step reflex

The tube feet are attached to the substrate, under ordinary circumstances, during the back sweep of every step reflex. The strength of this attachment (1) varies during the progress of the step reflex, (2) varies as a more or less constant multiple of the resistance to the step and (3) differs widely with different species of starfish.

The tendency to attach seems strongest during the first part of the step. Slipping and release does not occur with much frequency until after the tube foot has pushed back more than half way.

This conclusion is strengthened by an experiment on the very active tube feet of *Pycnopodia.* A large grain of sand was placed on one of the ambulacral disks. The step reflex which resulted was so violent that the sand grain was thrown, as from a miniature catapult, a distance of 4 or 5 cm. On repeating this the elevation, or angle of fire was seen to be such as would entail release of the pebble from the disk during the third quarter of the arc that the disk describes in back sweeping. I have not observed this vigorous catapulting activity on the part of other starfish (*Piaster*, *Asterina*, etc.).

To determine whether the strength of attachment is constant or whether it increases as the starfish pulls against increased resistance, the following experiments were undertaken.

One of the rays of an *Asterina* was tied by a long thread to a spring recorder which was calibrated to grams and set to write on a slowly moving drum. When the animal pulled against the spring, the strength of the pull was recorded as the height of the curve (*a-b*, fig. 9) above the base line. Now, when the animal had pulled the spring up to various heights, the glass plate on which it was walking was suddenly slid forward in the direction of locomotion. This resulted in an increased tension on the tube feet (recorded as *b-c*, fig. 9) which finally became sufficient to cause the animal to release hold on the substrate, when the writing point at once went down to the base line at *d*. Observations were made for various values of pull (*b d*) and their relation

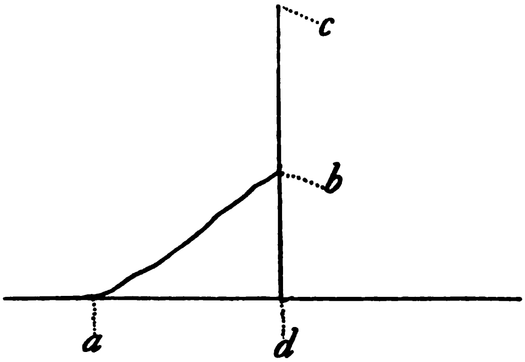

FIG. 9. CURVE MADE BY A CALIBRATED SPRING RECORDER ON A SLOWLY MOVING DRUM

a-b records the pull exerted against the resistance of the spring, *b-c* the additional tension which could be applied before causing the animal to release hold on a glass substrate.

to the strength of attachment (*c d*) computed. The results are shown in table 1.

Disregarding the high values of the first three observations, due observably to the fact that certain of the tube were "refractory," that is had not become coördinated in the step reflex and were simply attaching, we find that the strength of attachment of the tube foot, is on the average 2.7 times the amount of pulling the tube foot is doing at the time. That is to say the tube feet are attached strongly enough to resist a pull 2.7 times as great as that to which they are at that time subjecting themselves; a factor of safety against skidding of 2.7 on the smoothest surface. The value of friction in the above experiment was tested with the starfish inverted and found to be negligible (about 3 g).

TABLE 1

Relation of resistance to step, and strength of attachment of the tube feet in the crawling Asterina

RESISTANCE TO STEP (STRENGTH OF PULL) *b d*, FIGURE 9	STRENGTH OF ATTACHMENT *c d*, FIGURE 9	$\frac{c\,d}{b\,d}$
2 g.	15 g.	7.5
3	15	5.0
5	27	5.5
6	21	3.5
9	24	2.6
12	36	3.0
18 18 } 18 av. 18	45 57 } 54 av. 60	3.0
27	66	2.0
33	84	2.5
Average of last 8 observations....................		2.7

In *Pycnopodia*, the relationship is even more cosntant though it has a wholly different value. The figures are given in table 2.

The difference in the value of the figure is due to specific differences between the two starfish. It is not correlated with the relative ability of the tube feet of the two starfish to attach when not in the locomotor state, for the tube feet of a rigid *Asterina* (which attach tighter during locomotion) are much less firmly fixed to the substrate than those of a rigid *Pycnopodia*.

Whether the relation $\frac{c\ d}{b\ d}$ between the resistance to the step and the strength of attachment is constant, logarithmic or of some other nature can be determined only by a long statistical inquiry. It is fairly constant within certain limits.

Scheinmetz (1896) states that a starfish is able to exert a pull of 1350 g. in opening a bivalve, to which pull the bivalve gave way in short order under experimental conditions. His method of measuring the pull was directed rather to measure the strength of the attaching reflex, because he recorded the pull that caused a starfish to let loose its prey and not the pull which would overcome a maximal contraction of the longitudinal musculature of the tube feet. The amount of pull exerted by a tube foot, under conditions of locomotion at least, is as seen from the above figures from one-half to one-third of the strength of attachment at that moment.

TABLE 2

Relation of resistance to step and strength of attachment of the tube feet in the crawling Pycnopodia

RESISTANCE TO STEP (STRENGTH OF PULL) d, FIGURE 9	STRENGTH OF ATTACHMENT $c\ d$, FIGURE 9	$\frac{c\ d}{b\ d}$
9 g.	18 g.	2.0
18	33	1.8
24	60	2.5
30	60	2.0
36	72	2.0
Average		2.06

The strength of the step, or the pulling ability of different species differs widely. One ray of a small *Pisaster* was attached by a long thread to the spring recorder and the animal set to pulling against the thread. It remained oriented directly away from the recording apparatus for thirty-three hours during which time it was constantly pulling at a tension which rose with minor fluctuations to 225 g. It was at the end of the thirty-three hours when this maximum was reached. The arm then broke off at the thread, and the starfish, without showing any signs of fatigue, migrated very rapidly and quite continuously away from the injured arm. It was extraordinarily active for days after.

An *Asterina* of about the same size would only exert a pull of about 65 g. on the average of several trials. The peak of the curve was reached after about twenty minutes and the decline lasted two hours. The animal was not active after the pulling. Fatigue may be the cause of the quick decline as compared with *Pisaster*.

A very large *Pycnopodia* (50 cm.) which was extremely active and had according to Verrill's estimate (1914) 22,000 tube feet was tested in this same manner. The pulls recorded were 54, 45, 30 and 60 g. with time relations similar to those in case of *Asterina*.

This very low pull, for such a large and active animal, was thought to be due to the failure of the attaching reflex. The status of the attaching reflex could no be tested directly because there were tube feet that were "refractory" (attached and not undergoing the step reflex). Such tube feet would vitiate the significance of the point of release (*c*, fig. 9). Failure of attachment, however, was inferred because certain of the tube feet were observed to slip during the step and because when a weight was put on the back of the pulling animal, thus increasing the friction between the tube feet and the substrate, the value of the pull was increased. This was not the case when the weights were laid on the back of *Asterina* or *Pisaster* while these animals were pulling on a solid substrate but the value of the pull was increased by weight, when the attaching reflex was ruled out by putting the animals (*Asterina* or *Pisaster*) on sand.

The low pulling ability of *Pycnopodia* may be related to the fact that instead of pulling open its bivalve prey as do *Pisaster* and *Asterina*, it engulfs them as does *Astropecten* (Scheinmetz, 1896) which possesses no suckers on its tube feet at all.

SUMMARY

1. *Pisaster ocraceus* presents the three following well marked physiological states: (1) "locomotor," (2) "active but unoriented," (3) "rigid." The responses of the tube feet and arms differ markedly according to the physiological states of the animal. Other starfish studied present analogous physiological states.

2. Extension of the tube feet is spontaneous in animals in any of the three physiological states. An isolated tube foot inflated with water under pressure can be caused to slowly extend. The reaction is not quite normal.

3. Attaching is conditioned by the physiological state of the animal. An isolated tube foot carefully prepared and inflated with water will attach, and is more apt to do so if taken from a rigid starfish than if taken from a locomotor animal. Attachment may involve only a part of the ambulacral disk.

4. Withdrawal is a response to contact stimulation as is detaching under certain conditions.

5. The step-reflex intergrades with the withdrawal response as elicited by contact stimulation of the ambulacral disk. It is dependent upon contact stimulation and the presence of locomotor orientation which orients the step-reflex and conditions the tube feet to serve as a rigid "leg" during locomotion. The tube foot is attached most strongly during the first part of the step reflex. It is attached with 2.8 (*Asterina*) or 2.06 (*Pycnopodia*) times as much force as it exerts in pulling against resistance. The factor is fairly constant for various values of the resistance. The amount of force exerted in the step reflex varies markedly with different species.

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TEMPERAMENT AND BODILY CONSTITUTION

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Temperament can be defined as the general trend of conduct of an individual in response to stimuli. Bodily constitution is the structural make-up of the organism to which function must be added when activity is under consideration. The doctrine of psychology that temperament is largely a matter of bodily constitution is based on these premises.

McDougall (1) has grouped the temperamental factors into "two principal classes—on the one hand, the influences exerted on the nervous system and through it on mental processes by the functioning of the bodily organs: on the other hand, general functional peculiarities of the nervous tissues." If we alter the phrasing of the second designation to read: "on the other hand the influences exerted by the nervous system, and hence, in part at least, by the central processes on the functioning of the bodily organs," there is introduced the idea of a reciprocal or reversible reaction lacking in the original proposition and which includes the general functional characteristics of the nervous system as exhibited in differences of "excitability, rapidity of response,—fatigability and rapidity of recuperation." There will be adduced in support of the theory of the reversibility of the reaction—Bodily Constitution ⇄ Temperament, certain experimental data accumulated during the past two years, and an attempt will be made to demonstrate that the endocrine system largely directs the reversible reaction just outlined.

In view of the experimental findings to be utilized in this discussion, it is necessary to present a brief sketch of an idea concerning the relation of temperament to the expression of instinct. From the point of view of psychology, instinct is innate natural tendency. The physiological interpretation of those phenomena described as instinctive, as nothing more nor less than reflex actions with all the term "reflex" implies, affords a basis of understanding into which may be woven the conditioning influences of differentiation of function accompanying evolutionary development. It is unnecessary to go into any exposition of the part played by the phylogeny of the nervous system in the development of reflex mechanisms. When Gaskell's idea that the nervous system has been the dominant factor in evolution is considered from this point of view, it is seen that the domination is actually a facilitation for adaptation to environment, the efficiency of which determines the degree of advancement.

Now it is evident that with the differentiation of the tissues into organs of a predominantly specific function there occurred a simultaneously increasing influence of the nervous system on these new cell colonies. The resultant control of these special cell groups—which in turn may furnish hormones—of necessity gave rise to factors in the expression of reflex activity, with the consequence that the expression of instinct became more and more conditioned in mode or degree but not necessarily in its direction. There thus developed an increasing number of specific endogenous stimuli to which the organism reflexly responds as well as to the exogenous stimuli arising from without. There is little doubt that the sources and character of the exogenous stimuli are the same to-day as they were when organic living matter first made its appearance, as was established in principle many years ago by Lyell (2).

The organic differentiation then gave rise to new sources of stimuli which we have called the "endogenous" stimuli: these are distributed by the blood and apparently exert their effect through mediation of the nervous system or some of its subdivisions. Among the later developments of this differentiation there arose certain gland-like colonies of cells which produce secretions that have an effect upon the manner in which the organism responds to exogenous stimuli, and hence on instinctive reactions. Such secretions are produced for example by the thyroid, the adrenals, the gonads, the parathyroids, and may be collectively designated as the endocrine stimuli. It is with the appearance of these relatively recent structures that evidence begins to accumulate of the new factor of temperament in animal behavior, and this leads us to relate temperament with the activity of the endocrine glands. It cannot be denied that all the tissues probably participate in modifying the phenomena we recognize as temperamental, nevertheless there is ample support for the idea that these later special cell groups are very important conditioners of reflex activities as expressed in behavior. We see then how instinctive reactions, at first relatively simple reflex responses to changes in the environment, through the fundamental properties of protoplasm of irritability and conductivity, become more and more modified in expression, first by the development of the nervous system and then by the increasing differentiation of tissues, until it becomes a strongly conditioned reflex, on which the factors arising from the bodily constitution and represented largely by endocrine activity exert the dominating and directive forces.

We look on temperament therefore as largely the expression of the influence of the conditioning factors of the endogenous stimuli on instinctive behavior; the nature of this influence being determined by the directive stimuli arising from the products of endocrine activity.

Such a view maintains that bodily constitution plays a significant rôle in the genesis of temperament, but there has been very little experimental evidence presented supporting the idea that the reaction is reversible and should be written "Bodily Constitution⇄Temperament."

The studies of Cannon (3) with regard to the possibly greater activities of the adrenal glands resulting from emotional excitation, upon which some doubt has been cast by Stewart and Rogoff (4), but which the beautiful experiments of Maranon (5) tend to support in principle, were the early indications from the experimental side of alterations induced in the organism as the result of emotion—emotion here being understood to mean the affective aspect of instinct—while quite recently Buscaino (6) has published data which to his mind demonstrate deep-seated changes in structural form of several tissues following induced emotion.

Not only therefore can structural changes in organic constitution be caused by the production of exaggerated emotions, but also it can be shown that far-reaching functional alterations occur, for Hatai and I (7) have demonstrated that while the normal response of the isolated intestinal segment of the albino rat, when suspended in oxygenated Tyrode's solution at body temperature, is a contraction on stimulation by sodium carbonate: yet when the animal from which the segment is taken has been previously frightened or enraged, then the response to the stimulus, instead of being a contraction, is a relaxation. These experiments, however, while showing that the bringing into play of basal instincts has a marked effect upon bodily constitution, do not afford direct proof that what we designate as temperamental differences are immediately concerned, because any animal can be angered or frightened. Such proof is given, however, by the following two series of findings.

It is a matter of common observation that there are two extremes of temperament to be found, between which all gradations of behavior occur. Among persons there is a group of calm phlegmatic individuals with every evidence of a high threshold and a low irritability in their affective response to excitation; and on the other hand there is a group of excitable persons of low threshold and high irritability, whose reactions are out of all proportion to the importance of the exciting cause. These differences in temperament seem to be associated with differences in the manner in which the respective individuals handle their intermediary metabolism, since it has been found that the total variability of the soluble nitrogenous constituents of the blood as determined by analyses made from week to week is quite markedly different in the two groups (8). In the unemotional type, there is a tendency for the variability to be low. In the excitable type, on the other hand, the variability of the intermediary metabolism is relatively greater. Hence it is evident that as is one's temperament so is one's intermediary metabolism. Temperamental tone and intermediary metabolism are related. Here we have then a demonstration that temperament can affect bodily constitution.

A prettier proof is afforded by the fact that when the temperament of an albino rat is changed by gentling, from the condition where excitability and irritability are expressed by flight and pugnacity, to a state where the degree of expression of these instincts is reduced almost to insignificance, then the animal becomes markedly more resistant to the loss of the parathyroid secretion. Thus, out of every one hundred animals in which the emotions of fear and anger are conspicuous, some seventy nine per cent die of acute *tetania parathyreopriva* within forty-eight hours after parathyroidectomy, while when albino rats which have been gentled are similarly deprived of their parathyroids but thirteen per cent fail to survive this period (9). In addition it has been recently determined, although the results have as yet not been published, that the wild Norway rat, an animal which is still more excitable, is still more dependant upon the secretion of the parathyroid glands for its continued existence than either of the other two groups noted. It is not at all improbable that other factors than temperament in the narrow sense are concerned in the resistance of rats to the loss of the parathroid secretion, yet from these experiments the conclusion is justified that the differences in mortality are due to differences in temperament.

These studies extend the propositions developed by Cannon (2) with respect to the effect of emotion on bodily constitution to include the idea that factors of endogenous origin which give rise to temperament in turn have their function affected by temperamental responses, and the reaction "Bodily Constitution⇄Temperament is reversible and should be written as such.

They also indicate that of the endocrine glands the parathyroids at least play some part in the determination of the nature of the reaction, the exact office of which, however, is as yet uncertain.

The experimental demonstration of the profound influence of temperamental tone on bodily constitution is significant of the practical value of the regulation of the expression of temperament by voluntary action to the smallest variability in response consistent with adequate adaptation. Such a conception is interestingly developed in Osler's (10) essay "Aequanimitas" and seems to deny the rather pessimistic teaching of McDougall (1) that the natively determined factors of temperament "are but little capable of being modified by voluntary effort." Whether or not McDougall's opinion can be sustained is a matter of serious doubt when one considers the interreaction of the nervous system and the mental processes on the one hand, and the endogenous and exogenous stimuli on the other. One can hardly doubt the potency of voluntary effort in regulating the expression or effective aspects of temperament as understood in this discussion. If such were not the case our reactions would be permanently disordered and undirected.

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PRELIMINARY STUDY OF THE EFFECTS OF METHYL ALCOHOL FUMES ON BRIGHTNESS DISCRIMINATION IN THE WHITE RAT

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PROBLEM AND METHOD

The experiment reported in the present paper was performed in the psychological laboratory of the University of Kansas, and we are grateful to Professor Walter S. Hunter for his advice and criticism. The study was begun in March, 1919, and completed in June, 1919. The purpose was to determine by an objective measure the effects of the fumes of methyl alcohol on the brightness discrimination in the white rat when the alcohol in no way came in contact with the skin. It was hoped that the results of this experiment might explain, in some measure, the decreased visual acuity of which certain workmen who are compelled to labor constantly in the fumes of wood alcohol, complain.

Three sets of rats, consisting of eighteen animals in all, were used as subjects. The rats were all from the laboratory stock and were handled and fed for several days previous to training in order to insure gentleness. Each set included a single litter approximately a month old.

The apparatus consisted of the Yerkes discrimination box and brightness apparatus with movable stimulus adapter. The sources of light were two standard 25-watt mazda lamps placed 16½ inches from the stimulus plates. Heavy black cardboard was placed over the middle window of the stimulus adapter so that one alley of the discrimination box was dark. By placing a resistance coil and a Weston ammeter in circuit with the lights, the current could be adjusted to the number of amperes desired. The apparatus was set up in a dark room and the only other light besides the stimulus light was from a shaded lamp used in making records. This light was of small intensity and illuminated the discrimination box evenly throughout. As far as possible all external factors such as positions of the experimenters, noises, etc., which might either disturb the subjects or aid them in making a choice, were eliminated. Certain other precautions were taken during the course of the experiment, which will be described later.

The general method was to train the rats to discriminate between the standard light intensity (0.30 amperes) and total darkness. The light was the positive stimulus. Food and punishment were used during the early part of the experiment; but, because of bad effects, punishment was later discontinued. After the correct choice the rat was fed from the hand of the experimenter directly in front of the light. The animal was then picked up and put in the entrance door for the next discrimination. Ten trials at the beginning and later twenty trials were given daily. The stimulus adapter was shifted back and forth so that the light was first on one side and then on the other in irregular series. The criterion of discrimination between light of a given intensity and darkness was ten trials per day on three successive days averaging 90 per cent correct choices and on no one day less than 80 per cent correct. When an animal had succeeded in attaining this standard, the intensity of the stimulus light was decreased by reducing the current successively from 0.30 amperes to 0.20 amperes, then to 0.17 amperes, etc., as each discrimination was made. This was continued until it became impossible for the animal to set up the discrimination even after prolonged training. The intensity of the light was then increased to the point where discrimination could be made and then gradually decreased again. If with this practice the rat was still unable to reach the standard with light of a lower intensity, the intensity where the last discrimination was made, was taken as its limen for brightness vision.

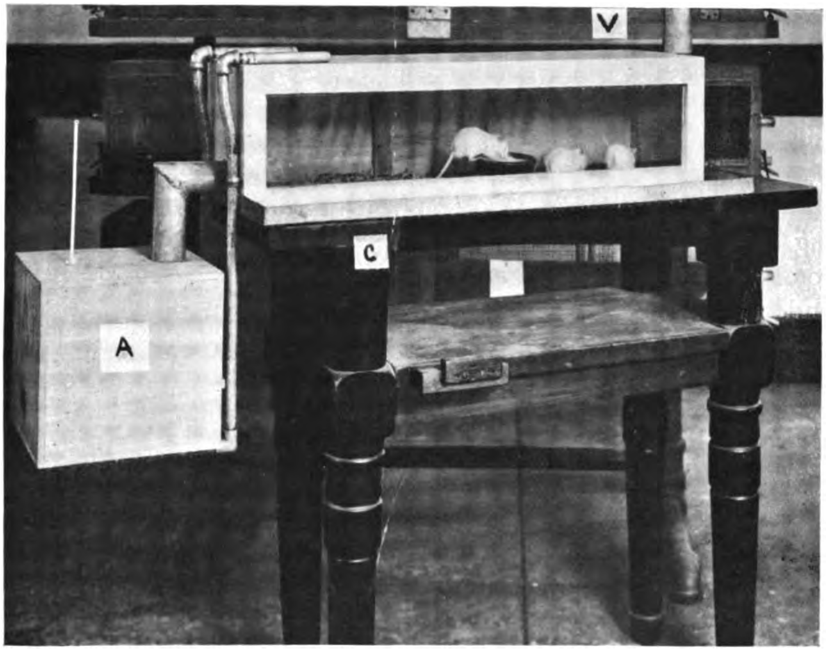

Fig 1. Alcohol Box

C, The cage in which the rats lived and through which the fumes passed; *V*, ventilator or exit for the fumes; *A*, box in which the fumes were generated.

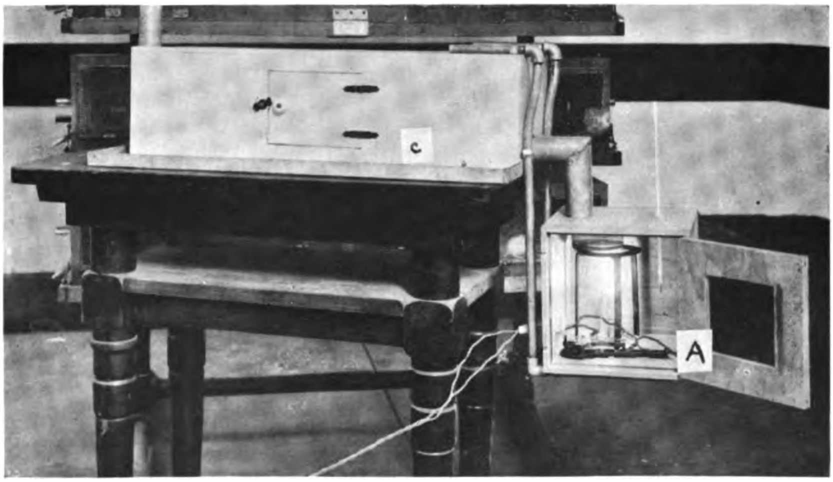

Fig. 2. Alcohol Box

The box, *A*, contains: the thermostat, thermometer, the electric light as a source of heat, and the alcohol upon a tripod.

As soon as this limen was reached the animal was placed in a cage through which the alcohol fumes were sent. This cage, as shown in figures 1 and 2, was essentially a rectangular wooden box 20 inches by 9 inches by 9 inches with glass in one side so that the behavior of the rats could be observed without disturbing them. The alcohol fumes were sent in from a box attached to one end of the cage and carried off by a ventilator on the other end. The fumes originated from pure methyl alcohol which was vaporized by the heat of a 100-watt Mazda lamp regulated by a thermostat to maintain a temperature of 30°C. in the case of set I and 50°C. in the case of set II. While living in the alcohol constantly, the rats of set I were taken out daily for a period sufficient to test them.

The time required for the first set of rats, consisting of five males and three females numbered from 1 to 8, to reach the limen ranged from 90 to 190 trials, and the rats were kept in alcohol continuously from a minimum of eleven days to a maximum of twenty-four days. Each rat, with the exception of number 8, which was apparently too sick to run, was tested at least three times and some of them as many as twelve and fifteen times to see if they could still maintain the limen they had reached. They were able to do so in every case.

All of the rats that were kept in alcohol continuously became sick. They were badly bloated, very inactive and at times refused to eat. With the exception of a few cases, however, none of the animals refused to attempt discrimination and usually succeeded in getting a large percentage correct. Four of the rats died while in the alcohol fumes but the four that lived soon seemed to become somewhat adapted to the fumes and improved in physical condition. These rats were taken out of alcohol for a time and kept in normal cages for a period not to exceed eleven days. During this time they were run every day in the discrimination box. When their physical condition had improved they were again put into the fumes for a period ranging from thirteen to seventeen days and thereafter were kept in the fumes for three hours per day only for the remainder of the experiment. Table 1 will show their progress from one condition to another measured in days.

The records of rats 1 and 7 may be taken as typical of the behavior of this set. Number 1 died after it was in the fumes for sixteen days while no. 7 lived throughout the course of the experiment. Counting the number of trials prior to the standard no. 1 took 60 trials to set up the first discrimination. The current was then decreased to 0.20 amperes and this discrimination was reached in 20 trials. With the current reduced to 0.15 amperes 30 trials were required. The rat was given 90 trials with 0.12 amperes but did not reach a percentage of accuracy greater than 60 per cent for any three consecutive days. The current was then increased to 0.13 amperes and the required standard was reached in 30 trials and for no day were there fewer than 70 per cent correct responses. It was therefore considered that this rat's limen for brightness vision was 0.13. It was then put in the alcohol fumes where it was kept continuously until five days later when it was taken out and tested at 0.13 amperes. A record of 60 per cent correct responses were made. It was again placed in alcohol and the following day made 100 per cent correct responses at the same intensity. Three days later, after having been in the alcohol continuously for nine days, it was able to make a record of 90 per cent correct choices. The rat at this time was bloated and moved rather slowly but there was no question as to its ability to make the discrimination. It was too sick to be tested after the ninth day and died sixteen days after it was placed in the fumes.

TABLE 1

RAT	NORMAL LIMEN*	NUMBER OF DAYS IN ALCOHOL PRECEDING RECORD COLUMN 4	LOWEST INTENSITY* TESTED IN ALCOHOL
1	0.13	8	0.13
2	0.13	7	0.13
3	0.13	6	0.13
4	0.13	15	0.13
5	0.13	14	0.13
6	0.13	15	0.13
7	0.13	20	0.13
8	0.13		

* Measured in terms of amperes.

Rat 7 was unfortunately upset by punishment and required 190 trials prior to the 30 required for the standard to make the first discrimination (at 0.30 amperes). With the current decreased to 0.20 amperes, 40 trials were needed but on no day were there less than 70 per cent correct; for 0.15 amperes only 10 trials, 80 per cent of which were correct, were necessary in order for the standard to be reached. With the current at 0.12 amperes there was no improvement after 50 trials so the amperage was raised to 0.13 amperes. The standard was then reached in 20 trials with no less than 80 per cent correct for either day. The animal was then put in the fumes and five days later tested 50 per cent correct but eight days later 100 per cent correct choices were made. This rat was kept in alcohol continuously for twenty-four days and after the thirteenth day was tested every day. During this period it succeeded in making the discrimination at its normal limen as shown by the last five daily records which were 70, 90, 90, 80, and 90 per cent respectively. After having been in the fumes continuously for twenty-four days rat 7 was placed in fresh air for two days, after which he was returned again to the fumes and kept continuously for thirteen days more. The first trial, taken on the first day of this period, the rat's record fell to 60 per cent when tested at its normal limen (0.13 amperes), but the succeeding six trials on each of the following days had a record of 90, 90, 90, 100, 100, and 100 per cent respectively. This record was maintained throughout the following days both during the time that the rat was in the alcohol continuously and during the period that he was kept in it for three hours per day only. The records for these three hour periods will be discussed later.

It will be noted that rat 1 made 60 per cent and rat 7 made 50 per cent correct responses when tested for the first time after they were put in the alcohol fumes. Both, however, had regained their normal efficiency by the second trial and maintained an excellent record thereafter. This was true of every rat of set 1. Rat 3 fell to 40 per cent correct, no. 4 to 70 per cent, no. 5 to 60 per cent, no. 6 to 70 per cent, and no. 2 to 60 per cent correct responses. With the exception of rat 7, the rats which survived of set 1 made the same type of failure the first trial after they were returned to normal conditions. By the second trial they were responding with the same high percentage of correct responses that they had maintained in the alcohol fumes. It would therefore seem that their low records were due to the change in environment and not to the alcohol fumes. These records are shown in table 2.

Because of the bad physical effects noted above the method was changed with the remaining rats, set 2. Instead of putting them in the fumes to live, they were put in the alcohol box for three hours per day only, but in very strong fumes, the thermostat being regulated to maintain a temperature of 50°C. as opposed to 30°C. in the tests of set 1. The results were, however, exactly the same as those secured with the previous method. Every rat could make the discrimination at which it last succeeded in normal condition.

Set 2 was composed of 5 rats, 3 males and 2 females, born March 7, 1919, and set 3, of three rats, 2 males and 1 female, born April 5, 1919. Since the same method was used with these rats they will be considered as a single group. Rats 4, 5, 6, and 7 of set 1 were also given the treatment outlined in the above paragraph after it was begun with the latter rats. The rats of set 2 were exceedingly timid and sensitive to punishment so it was discontinued with them and for the remainder of the experiment. The record of rat 9 may be considered a fair example of the behavior of the rats of this group. Two hundred trials were necessary for this rat to set up the first discrimination due to the fact that 40 trials were necessary for the rat to recover from the bad effects of punishment administered the 11th day after the rat had a record of 90 per cent for the two preceding days. No trials were needed to maintain the standard when the current was reduced successively to 0.20 amperes, 0.17 amperes, and 0.15 amperes. It took 30 trials to make the discrimination at 0.13 and 10 trials at 0.12. Seventy trials were given at 0.11 but no 30 trials ever averaged more than 60 per cent correct. When the current was again raised to 0.12 amperes 100 per cent correct responses were made the first day. The rat was then placed in the alcohol fumes for three hours a day as described above. It was tested every day for the next 26 days and was able to make a high percentage of correct choices, the last ten trials being 100, 80, 80, 70, 70, 80, 80, 100, 90, and 90 per cent respectively.

Rat 12 of this group attained the lowest limen of any of the rats tested. One hundred trials were needed for the initial learning and the current was successively reduced to 0.25 amperes, 0.20, 0.17, and 0.15 amperes without extra trials. It took 20 trials to master the discrimination at 0.13, 10 at 0.12, and 20 at 0.11. With the current reduced to 0.10 the standard was reached in 40 trials. The rat was tested with the current at 0.09 but refused to make a discrimination. Wher the current was raised to 0.10 he made the choice again as usual. Control tests were made to discover whether the animal was making a determination on any other basis than the stimulus light. The light used for taking records was turned out, the stimulus adapter was moved back and forth several times between trials as it was thought the rat might be discriminating on a basis of noise, though this would of course not explain why the rat failed to discriminate at 0.09. The stimulus light was left always on one side or the other, since it was possible that the animal had a complicated alternation which gave him a high percentage of correct responses. The time interval was made exactly the same between successive trials. None of these controls, however, broke down the response. When no light was used the rat refused to make a decision.

Table 2 below shows a complete record in terms of days for rats 4, 5, 6 and 7, the remaining rats of set 1.

The results from these rats have been discussed with the exception of those obtained during the period that they were in

TABLE 2

RAT	DAYS REQUIRED FOR LEARNING	IN ALCOHOL	NORMAL	IN ALCOHOL	IN ALCOHOL THREE HOURS PER DAY
4	32	14	6	17	18
5	20	21	11	13	24
6	26	18	11	13	18
7	32	24	2	13	21

alcohol for three hours per day as indicated in the sixth column in the table above. During this time each rat in this group maintained the standard of accuracy which he had obtained. The percentage of correct choices during the last three trials were as follows: Rat 4—90, 100, 100 per cent; rat 5—80, 90, 100 per cent; rat 6—100, 100, 80 per cent; rat 7—100, 80, 90 per cent.

The results of set 2 are shown in table 3.

TABLE 3

RAT	NORMAL LIMEN*	NUMBER OF DAYS IN ALCOHOL, THREE HOURS PER DAY ONLY	LIMEN AFTER EXPOSURE TO ALCOHOL FUMES
9	0.12	25	0.12
10	0.11	28	0.11
11	Died		
12	0.10	28	0.10
13	0.12	23	0.12
14	0.13	14	0.13
15	0.13	18	0.13
16	0.11	19	0.11
17	0.13	7	0.13
18	0.13	16	0.13

* In terms of amperes.

CONCLUSIONS

It may be believed that sufficient allowance has not been made for practice. It must be noted, however, that in the beginning trials where the problem was to determine the normal limen that every rat was given a number of trials where the stimulus used was a light intensity below that at which the animal had succeeded. In the case of rat 1, as noted above, 90 trials were given and at the last ten trials the rat only got 60 per cent correct. This was true of every rat. Furthermore several rats were tested after they had been in the alcohol fumes and they were never able to reach a lower limen than they had reached in normal conditions. All of the rats of the first set and several of the second set were tested in this way. It is possible of course that practice was merely counterbalancing the effect of the fumes. It does not seem, however, that it could be exact in every case.

To be certain of this point it would be necessary to determine the effect of continued practice of rats kept in normal conditions. It is also possible of course that the alcohol caused certain defects which, because of slow progress, was insufficient to cause any change in the accuracy of the responses within the limit of this experiment. Anatomical study alone would determine this. Further study should also include the effects of contact and feeding. At this time within the limits of a preliminary experiment we may only say that such results as we have obtained would seem to show that the alcohol fumes, administered by our method do not cause a decrease in the brightness sensitivity of the white rat.

BOOK REVIEWS

Human Behavior. By Stewart Paton. New York, Charles Scribner's Sons. 1921. pp. 464. $6.00.

The literature of comparative, social, educational and general psychology; of the various special fields of theoretical, experimental and clinical psychopathology, including psychoanalysis; of psychobiology, endocrinology, genetics and general biology, and of that group of technologies to which we commonly refer as "medicine" may safely be regarded as containing material for the construction of a systematic account of human behavior. It is, perhaps, the most significant feature of Dr. Paton's book that he has drawn freely from each of these sources and has, nevertheless, given us a good deal more than a mere anthology. He quotes—often extensively—from nearly four hundred different authors whose views or findings contain material for the behaviorist's consideration, and one feels only here and there that a given quotation has not sufficient relevancy to the author's theme to justify its inclusion. Wherever the reviewer has been able to consult the orginal sources of these apparently irrelevant quotations it has seemed that Dr. Paton's only fault in the matter has been his assumption that the reader will have read and remembered their contexts.

The author presents his book as an introduction to the study of human behavior and, more broadly, as a scientific text-book for "parent, teacher, physician, student of social phenomena, prospective reformer, statesman and philosopher" each of whom has to deal with special problems in human behavior. He states, in the first chapter, that, "One natural and obvious method of taking up the problems of human behavior and conduct is to proceed from the consideration of simpler phenomena as observed low down in the animal series, or from relatively simple activities of the insane, to complex functions of the normal person." There follow a chapter on "Adjusting Mechanisms" and one on "Special Mechanisms of Adjustment," in which he traces, with much necessary abridgment, the phylogenesis of the human reactive equipment. A chapter on the development of the personality gives in outline what might easily be expanded into a valuable treatise on the ontogenesis of human behavior.

Most students of human behavior adopt one of three different attitudes toward Freud's psychology: (1) enthusiastic, fundamentally uncritical acceptance of it, along with a joyous conviction that whoever rejects it is enslaved by a dominating "Unconscious;" (2) a cautious, middle-of-the-road acceptance of it, which leads to a more or less vague paraphrasing of its principle dogmas as these currently appear and are acclaimed by the orthodox, and (3) impatient rejection of it on the grounds that the whole Freudian movement is based upon a rejection of the painstaking, critical, soundly progressive methods of the natural sciences. Dr. Paton is a psychopathologist who discloses a very broad grasp of what the phylogenesis and ontogenesis of the total human reactive equipment signifies for an explanation of human behavior, hence his attitude toward Freud's psychology is a matter of importance. Throughout the book his terminology and his exposition of the determinants of human behavior clearly reflect the influence of the psychoanalysts. In a chapter which deals with conflicts and dissociations of personality he quotes freely from Ernest Jones, and with apparent acceptance of the Freudian view of the matter. In fact, he seems to appropriate much psychoanalytic terminology and a considerable number of psychoanalytic concepts without reluctance and with no desire to patonize such appropriations by parapharsing them. Nevertheless he never falls away from the statement, in the second chapter, that "three primitive 'drives'—one the tendency to preserve the physiological balance, the second, to secure the safety of the individual, and the third, to insure the perpetuation of the species—are the dominant forces in life.'' In another chapter he repudiates, in effect, the orthodox psychoanalyst's central concept, viz., that "the sexual life of a person is the nucleus about which all other mechanisms of adjustment are grouped."

Endocrinology, like psychoanalysis, has its exponents who find their guesses so alluring that they present them as established facts, but the importance of the glands of internal secretion as parts of the human reactive equipment is too great to justify the behaviorist in rejecting the good with the bad in this matter. Dr. Paton has done wisely, perhaps, in keeping his references to the literature of Endocrinology more or less detached from the main body of his argument.

The determinants of the various troublesome and distressing features of old-age maladjustments include, according to the author, reaction to the failing sense of adequacy, breaks with reality in order to dodge uncomfortable situations, "wishful thinking," relaxation of the censorship which has previously concealed some traits of character, etc. The chapter devoted to this subject is, like the book as a whole, too technical for the lay reader, but it is so sound and practical as to deserve separate presentation in popular form.

A chapter on "The Intelligent Direction of Activities," which deals with education, finds the author in radical disagreement with current educational methods. There is nothing new in his contention that the aim of education should be biological and not academic; but he impresses the reviewer as being exceptionally clear and productive in his explanations of what he means by this.

The book, as has already been stated, is too technical for the lay reader, and its real usefulness is to be sought in its exposition of problems in behavior which, although of urgent practical concern to the medical clinician, the educator, the social worker, etc., demand an enormous amount of patient research on the part of well-trained laboratory workers. Regarded as a challenge to the laboratory man to direct his investigations toward the development of a productive science of human behavior, Dr. Paton's book is a valuable inventory of what is known concering this subject and of the many important objectives which should engage the attention of investigators in various fields of research.

G. V. HAMILTON.

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